# ENDEAVOUR

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# ENDEAVOUR

The British quarterly scientific journal ENDEAVOUR was first published, by Imperial Chemical Industries Limited, in January 1942. Its purpose is to provide scientists, especially those overseas, with news of the progress of the sciences. While emphasis is laid upon British work, narrow insularity is avoided by publishing a number of articles from overseas contributors and by impartial reference to the world's scientific literature. To make the journal truly international in character it is published in five separate editions—English, French, German, Italian, and Spanish.

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*The drawing on the cover is of the bark* Endeavour, *which, commanded by Captain James Cook and carrying a number of scientific workers, was sent out by the British Admiralty in* 1768 *to chart the South Pacific Ocean and observe the transit of Venus*

# ENDEAVOUR

A quarterly review designed to record the progress of the sciences in the service of mankind

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# Some limits to popular science

Travellers in foreign countries seem sometimes to believe that they will be perfectly understood if they speak loudly and clearly enough in their own language. Such a fancy is harmless enough, but it rightly makes ridiculous those who display it. Yet there seems to be growing up a similar belief which is scarcely less ridiculous but not entirely harmless—a belief that all the mysteries of science can be made clear to the layman if only scientists will take the trouble to explain themselves in very simple terms. In the circumstances of today it is undeniably desirable that the general public should have an appreciation of science in the widest sense—its discipline and its methods, its results and applications, its promises and its dangers, its organization and its practitioners. This aim is laudable, but no useful purpose is served by propagating the belief that there are no limits to the success which may be achieved.

Clarity of exposition is certainly of the greatest possible assistance here, as in the communication of any other kind of knowledge, and it is undoubtedly true that scientists have much to learn not only in explaining themselves to the layman but in explaining themselves to each other. Nevertheless, there are limits to what the most lucid thinker and writer can do when seeking to enlighten untrained and often unreceptive minds. This is true of all complex human activities, and it is curious that while it is so generally recognized in many other spheres it is so often ignored in discussing the popular exposition of science. An analogy can perhaps fairly be drawn from the ancient and widely played game of chess. All the world may appreciate that it is skill and patience which lead to victory or rejoice in the success of their local or national champion. But no one would suggest that the spirit of the game, the danger of a particular situation, the precise point at which the play turns in favour of one player or the other, and all the fine points mastered only after years of play, can be appreciated by those who do not know even the basic moves assigned to the pieces. This fact is accepted by those seriously minded newspapers which regularly have columns on chess—such columns are always written in a style designed to convey as much information as possible to the enthusiast, but make no pretence of catering for the uninitiated. This does not, of course, mean that nothing interesting can be said about chess in more popular publications. There, however, the emphasis must rightly be placed upon those limited parts of the subject which a wider readership will find interesting and intelligible. The result itself, the idiosyncrasies of the players, unusually brief or protracted play, can all be touched upon. But none would suppose that this kind of comment tells us wherein lies the absorbing interest of the game for those who play it or anything of the complex tactics which determine success.

The analogies can be multiplied almost endlessly. Can the tone-deaf appreciate a fine orchestral performance, or any but a trained jurist appreciate the subtle points of a complex legal argument? Again, however, there are parts of such matters which anybody with an inquiring mind can grasp even in the short time it is possible for the ordinary person to devote to the pursuit of, to him, relatively minor interests. Similar arguments can surely be applied to the popular exposition of science. Parts of the subject must surely remain a closed book to the layman virtually by definition; when he can read these pages he is no longer a layman but entitled to call himself a scientist, whether he reaches this status by institutional or private study. This is a fact implicitly recognized by all the most successful writers of science for laymen: their genius has been not in explaining science as a whole, but in explaining those parts of it which can be made intelligible and interesting to the ordinary person with very limited time to spare.

Generally speaking, unfamiliar matters, whether scientific or otherwise, can be understood so far as the reader can relate them to his own general knowledge and experience. He can appreciate the general nature of many major scientific discoveries, and he can understand their likely effect on his daily life if they were to be widely applied. That he ought to have this kind of knowledge is generally agreed. A high rate of technical development seems, whether we like it or not, to be the only answer to the acute problem presented by a world population that is both increasing rapidly and clamouring for a higher standard of living. To achieve this development, the widest and speediest possible dissemination of technical knowledge is essential, since the necessary decisions on policy are still largely in the hands of non-scientists.

Even on the purely factual side of science, however, there are many important discoveries which

it is virtually impossible to explain to the layman in anything but the most superficial way, for they are too remote from his experience and ordinary modes of thought. It is, for example, surely no accident that while the major applications of chemistry are quite widely known, for they form part of our daily lives, the whole realm of theoretical chemistry has been virtually untouched by the popular expositor. The elegance of an ingenious organic synthesis or the nature of the dynamics of a complex reaction cannot be conveyed to those with no prior knowledge. The public, for example, may accept the fact that the elucidation of the structure of vitamin $B_{12}$ is a great achievement, but they cannot be expected to see wherein its greatness truly lies.

There are other barriers to popular understanding. Science would be an uninspiring business if it consisted of no more than the collection of new facts and their application for practical ends. These are but the beginning and the rounding off of a complex process. To be manageable, facts have to be marshalled within the limits of general laws, from which in turn new facts may be deduced. Practical applications almost invariably call for the solution of problems scarcely less difficult—and, indeed, often more difficult—than those involved in the original discovery. Many non-material factors also are involved: imagination to see the significance of facts, pleasure from the pursuit of knowledge for its own sake, the satisfaction of the creative impulse. It is surely unduly optimistic to hope that mere clarity of writing will suffice to convey to the layman a proper understanding of the whole complex intermingling of the material and the abstract which constitutes modern science. How long is it seriously supposed he can give to reading about this vast subject? Surely it is at the very best no more than will enable him to scratch at the surface, for even the individual scientist can today apply himself to only a very small part of it.

In talking of the popular exposition of science it thus seems easy to confuse the part with the whole, to suppose that the present vast outpouring of popular scientific material represents all aspects of scientific activity. The point is not merely of academic interest; it is of considerable practical importance also. For better or for worse, scientists have a great, and still rapidly increasing, influence on world affairs; one can see no limit to the material achievements of science. Not surprisingly, there are grave misgivings about this, for it is only too apparent that evil as well as good can spring from scientific progress. That the great majority of scientists are interested in applying their work only to beneficial ends may be well known in scientific circles, but it would be complacent to suppose that all the world is convinced of this. Nothing could be more damaging to good relations between scientists and the public than a suspicion that they are deliberately hiding knowledge of their activities by describing them in language so esoteric and obscure as to be incomprehensible to any but their fellow experts. It would be far more useful, and less productive of disillusionment, to teach that science is in reality a strict discipline demanding patient application. If he really wants to understand science, and not merely its more obvious results and applications, the layman must be prepared to study it seriously. How this is to be achieved, however, in an age in which there are so many interests to occupy hours of leisure, entails argument far too controversial and complex to be embarked upon here. It does seem, however, that the time has come for more general introduction of science into primary education.

In the years to come, the world will inevitably turn more and more to science to solve the grave practical problems which beset it. The response will certainly be wholehearted, but one would have to be very optimistic to believe that there will be no disappointments. These will be all the greater if it is believed that science is really a relatively simple matter, for it could make failure seem the result of indifference or preoccupation with more sinister research, rather than of the intrinsic difficulty of the problems themselves. A limit to the extent to which science can help the world is set by the number of people who are able and willing to give the time and trouble necessary to acquire scientific knowledge appropriate to their purposes. This applies, of course, not merely to science but to all learning: superficial knowledge will never be an effective substitute for real understanding. It is rather easy to forget that it is not on science but on scientists that we depend for the solution of some of our pressing problems.

# Quantum mechanics and common sense

ALFRED LANDÉ

The belief has grown up in recent years that quantum rules are so fundamental that they must be looked upon as axiomatic and incapable of explanation in general terms. The present article argues that this defeatist attitude is unnecessary and that an understanding of quantum mechanical axioms can in fact be gained from a logical approach to the subject, based on the fundamental assumption of continuity of cause and effect.

Quantum mechanics has the reputation of being incomprehensible to all but a select group of theoretical physicists schooled in complex mathematical techniques and trained in the art of abstract reasoning. This reputation is enhanced by a strange dualism between corpuscular and wave-like manifestations of matter, by an uncertainty replacing the determinacy of causal chains, and by a complementarity preventing us from forming any definite image of the microphysical world and asking us instead to be satisfied with a mathematical formalism capable of embracing a variety of phenomena of an apparently contradictory character. When penetrating further into the sanctuary of quantum theory we are confronted with revolutionary hypotheses, today hardened into axioms, such as the Planck oscillator rule, $E = h\nu$, or the modern quantum prescriptions of Schrödinger and Born-Heisenberg 'involving strange ideas and some processes of calculation which cannot in detail be understood' (J. Lennard-Jones [2]). The novice is supposed to accept the quantum rules at face value 'because they work'; later on, when he has practised them for years, he may even become oblivious of their problematic character. Or, if he preserves his open mind, he may agree with P. W. Bridgman [1] that 'when we go far enough in the direction of the very small, quantum theory says that our forms of thought fail, so that it is questionable whether we can properly think at all . . . Eventually we shall have to find better ways of thinking.' This is a rather distressing state of affairs after twenty-five years of duality, uncertainty, and complementarity. Shall we accept for ever an array of unintelligible formalistic rules which happen to work, or will it be possible to conceive quantum theory as a manifestation of simple physical principles?

A hopeful note to this effect has been struck by Bridgman in the article already cited [1]:

> 'I believe the experts would at present agree that whatever new way we devise to think about the microscopic universe, the meaning of our new concepts will have to be found back at the level of the large-scale events of daily life, because this is the scale on which we live our lives, and it is we who are formulating the new concepts. This recognition and agreement entails, I believe, a consequence that is not commonly appreciated, namely, that the seeds and sources of the ineptness of our thinking in the microscopic range are already contained in our present thinking in the large-scale region and should have been capable of discovery by sufficiently acute analysis of our ordinary common-sense thinking.'

These are prophetic words indeed; and the prophecy has to a certain extent come true, as the present article intends to show. An understanding of the enigmas, now dogmas, of quantum mechanics can indeed be gained through a common-sense approach, and could have been gained ever since Schrödinger, Born, and Heisenberg succeeded in merging the incoherent quantum rules of the older theory into a consistent structure. But the road to a common understanding has been obstructed by the historical development, which derived its impetus more from spectacular discrepancies between microphysical observation and classical expectation than from a critical analysis of the intrinsic failings of the classical theory itself. With the sole exception of the Bohr correspondence principle, the emphasis was always on the dramatic and revolutionary, and it had to be so in order to justify the break with the Newtonian tradition. Then at last it all seemed to condense in the Schrödinger equation, today placed at the entrance gate to quantum theory. But this is like beginning a study of electromagnetism with the Maxwell-Lorentz field equations rather than with moving magnets and coils of wire, or mechanics

with $E = mc^2$ rather than with falling apples and swinging chandeliers.

Another obstacle in the search for a simple approach to quantum theory has been the current philosophy that the task of physics is to find a rational description of the interrelations between phenomena, and that the quest for an explanation is futile. Although this may be true in the last analysis, it can hardly be said that modern quantum theory is yet in a state of representing last and irreducible principles. To accept duality, uncertainty, and complementarity as inscrutable articles of faith may have had its advantage in the beginning when physicists were groping for any solid ground to build upon. But to persist in this attitude is sterile in the long run and dangerously close to medieval scholasticism. Fundamental knowledge has never yet made progress at times when the return to common sense was received with a condescending smile at such naivety.

The following attempt at an 'acute analysis of our ordinary common-sense thinking' will start from considerations which seem at first sight to have little to do with quantum theory, since they do not even contain any mention of Planck's constant $h$. They try rather to exploit a common experience, namely, the fundamental fact that the effect should be a continuous function of the cause, or that an infinitesimal cause should never be able to produce a finite effect. Our programme is to show that such a continuity of cause and effect, which is a common experience, implies that on a microscopic scale there must occur discontinuous events which are controlled by probability rather than by cause-effect chains (see section I). The question then arises whether there may perhaps be definite laws connecting, if not individual events, at least statistical groups of events. But such groups, too, are only loosely connected, and there is no single-valued relationship between the probabilities of various events. Instead of trying to formulate the ambiguous relationship between definite probabilities, it is convenient first to introduce ambiguously defined 'probability amplitudes,' $\psi$, which are nothing but vectors giving a direction to the transitions from one state to others, and are interconnected by a well defined relation law (see section II). We hope to make the essential points of these considerations understandable by means of a familiar geometrical analogy, and to lift the curtain of mystery which has always shrouded the $\psi$'s because of the customary representation by complex symbols.

Sections I and II present a fundamental but empty framework of formal relations between probabilities, which must be filled with concrete substance, namely, with a dynamical theory of actual mechanical systems possessing energy, momentum, and position in space and time. It is only here that the quantum $h$ makes its appearance (section III). Planck's constant was introduced first in radiation theory through the oscillator rule $E = h\nu$, then in Bohr's quantization of orbits, and finally in the improved quantum prescriptions of Schrödinger, Born, and Heisenberg. But all these rules were invented *ad hoc* for the purpose of establishing a fundamental periodic relation between energy and time, and between momentum and space co-ordinates. This periodicity has always been, and still is, the most puzzling feature of micromechanics, although the experts, after long years of using it daily, seem to have forgotten its highly problematic character. Thus, after fifty-five years of quantum research, the question of why an oscillator emits energy quanta $h\nu$, formerly answered by 'because the Planck equation says so', is now answered by 'because the Schrödinger equation says so', in agreement with the experimental facts. It is the object of this article to point out that a more satisfactory reply can be given if one will try to lay bare the simple and elementary roots of the quantum theory.

## I. TRANSITIONS BETWEEN STATES

We start from an analysis of the contrast between equality and inequality of physical objects. Imagine two gases, composed of the same kind of particles. Those of gas $A$ may be of spherical shape, those of gas $B$ may be distorted to ellipsoids. Are these two gases alike or unlike? Better, are they distinguishable or indistinguishable? Still more realistically, are they separable or inseparable by some sort of selective device if the two gases are mixed? The expected reply will be: When $A = B$ or, in our example, when the ellipsoids degenerate into spheres, then $A$ and $B$ are indistinguishable and inseparable. But when $B$ differs physically from $A$ even to an ever-so-small degree, then they are unlike and separable, at least in principle. However, this reply is in serious conflict with the general experience that the effect is a continuous function of the cause. Indeed, imagine a selective device—call it an $A$-filter—which passes the $A$-particles and blocks the $B$-particles, or is capable in some other way of separating the $A$'s from the $B$'s. Now suppose the difference between $A$ and $B$ to be gradually decreased; the $A$-filter still blocks the $B$-particles.

But the moment when $B$ is made exactly equal to $A$ then the $A$-filter will suddenly pass all $B$'s. That is, an infinitely small cause, the changing of the difference between $A$ and $B$ from ever-so-small to exactly zero, would have a finite effect, a change from none passing to all passing. One feels intuitively that such an abrupt change of the effect is 'unphysical', that filters discriminating in such a drastic fashion between ever-so-small and exactly zero cannot exist in nature. The serious difficulties which such filters would entail in the domain of thermodynamics (second law) were pointed out by J. W. Gibbs in the 1880's. But Gibbs did not make the positive inference, which would have brought him to the doorstep of quantum theory, that hence there ought to exist a continuity of intermediate cases between $B = A$, where all $B$'s are passed by an $A$-filter, and $B$ being quite different from $A$ (written $B \neq A$) where all $B$'s are blocked by the same $A$-filter. There ought, indeed, to exist a continuity of possible states $B$ of a 'fractional equality' with $A$, written $B \sim A$, such that the particles in the state $B$ neither all pass nor are all rejected by the $A$-filter. $B \sim A$ would signify that a certain statistical fraction of the $B$'s will pass and the remaining fraction will be rejected by the $A$-filter. This is, indeed, the only way of establishing continuity between none passing and all passing when the state $B$ is gradually made to approach the case $B = A$, and thus avoiding the thermodynamic paradox of Gibbs.

The two limiting cases $B = A$ and $B \neq A$ (the latter also written $B =$ *non-A* or $B = \bar{A}$) are illustrated in figures 1($a$) and 1($b$). The intermediate case $B \sim A$ is shown in figure 1($c$). The statistical passing-fraction $P$ in the latter case may be taken as the operational definition of the fractional equality between the states $A$ and $B$. The same passing-fraction $P$ then represents the probability fraction for an individual $B$-particle to pass, and $1 - P$ the probability fraction of $B$ being rejected.

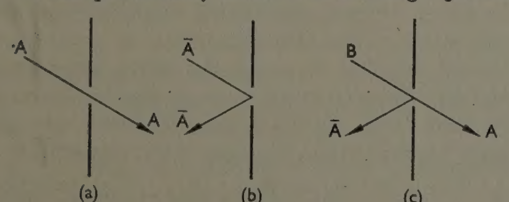


FIGURE 1

Without appealing to *ad hoc* hypotheses and without using microphysical evidence, but merely on grounds of the postulate of continuity of cause and effect, we have thus arrived at a basic tenet of the quantum theory, namely, that there must be states $B$ of a mechanical system (particle, atom, etc.) intermediate between $A$ and *non-A* such that an $A$-passing device will neither pass all $B$'s nor reject all $B$'s, but will transmit or reject $B$'s according to a probability ratio, $P/(1 - P)$.

Whenever we see a group of particles passing an $A$-filter, then we are entitled to conclude that the particles of this group are in the state $A$ so that they will pass an $A$-filter again and again, unless perturbing causes change their state $A$ to some other state. On the other hand, we had come to the conclusion that, in case of $B \sim A$, only a certain statistical fraction $P$ of the incident $B$-particles will pass the $A$-filter. But according to what was said just before, this entire group of $A$-passing particles must now be in the state $A$. Hence this group must have carried out a transition, must have 'jumped' from their original state $B$ to the new state $A$ (and then remained there unless further perturbed). These jumps from $B$ to $A$ may be ascribed to the reaction of the $B$-particles to the $A$-filter which, after turning them from $B$ to $A$, will then let them pass. But similarly, the remaining fraction $1 - P$ of incident $B$-particles which is rejected must have jumped in contact with the $A$-filter to the state $\bar{A}$ and remain there unless further perturbed. The newly acquired states $A$ and $\bar{A}$ in the passed and rejected group respectively are indicated in figure 1($c$). The plausibility that this figure represents the effect of the $A$-filter on incident $B$-particles is enhanced by the consideration that one could hardly think of any other way of satisfying the continuity principle, with figure 1($c$) representing cases intermediate between the limiting cases of figures 1($a$) and 1($b$). The passing-fraction of $B$-particles through an $A$-filter (figure 1($c$)) then becomes the probability of transition from the original state $B$ to the new state $A$ in contact with the $A$-filter. We denote this probability as $P(B \rightarrow A)$ or shortly as $P(B,A)$. Reasons of thermodynamic equilibrium require that $P(B,A)$ equals $P(A,B)$, i.e. the passing-fraction of $B$-particles through an $A$-passing filter is as large as the passing-fraction of $A$-particles through a $B$-passing filter. This symmetry of the passing-fractions and transition probabilities is an important feature of the quantum theory.

Consider an example. Let A stand for a state of position of a particle within a small volume element surrounding a certain point in space, and let B denote a certain energy value of the same particle, e.g. of the electron in a hydrogen atom.

Suppose the electron has been ascertained to be in the energy state B and is now tested with a gauge having a hole (symbolically speaking) at the place A. The particle will 'pass' with probability $P$ and will be blocked with probability $1 - P$.

Another interesting case is that of B being a state of orientation of a silver atom parallel to the $z$-axis, the $A$-filter being an inhomogeneous magnetic field of $z'$-direction making an angle with the $z$-direction. This 'filter' of Stern and Gerlach compels the incident atoms to choose either the $+z'$-orientation and pass, or the $-z'$-orientation and not pass, i.e. to emerge in a different direction, so that the atoms in the new states $+z'$ and $-z'$ are separated.

A great deal more ought to be said about those selective devices or filters and about the various states; the latter are always meant to be well defined states. For example, a state of position at $xyz$ of a particle is not a well defined state, but position at $xyz$ at a certain time $t$ is. In this short article there is no room to elaborate on the fact that only well defined states are interconnected by definite probability fractions $P(A,B)$ = transition probabilities. Most filter tests are *Gedanken*-experiments hardly capable of being carried out directly; the importance of the Stern-Gerlach experiment was that it yielded the first direct confirmation of the actual existence of distinct quantum states and their statistical interrelation.

In general, a state is in a relation of fractional equality, $0 < P < 1$, to other states, and cases of total inequality, $P = 0$, are the exception. This fact can be used for bringing a certain order into the chaos of all (well defined) states of a mechanical system. From an indiscriminate list of all states one may pick out a set of states denoted as $A_1A_2A_3 \ldots$ which are mutually quite unequal, i.e. separable by filters, or, to use the technical expression, are mutually orthogonal. From the remaining states one may pick out another set of mutually separable or orthogonal states $B_1B_2B_3 \ldots$, then a third orthogonal set of states $C_1C_2C_3 \ldots$, and so forth, until all states of the mechanical system (particle, atom, molecule) are arranged in orthogonal sets:

$$\left.\begin{array}{l}\text{Set A: } A_1A_2A_3 \ldots \\ \text{Set B: } B_1B_2B_3 \ldots\end{array}\right\} \ldots\ldots\ldots \text{(I}a\text{)}$$

and so forth. Whereas a state $A_k$ is totally different (separable) from any other state of the same set $A$, there are fractional equalities $P(A_k,B_j) < 1$ between the states of the set $A$ and those of the set $B$, measurable as transition probabilities in statistical experiments with filters as described before. The following table lists the passing-fractions or probabilities of transition between the states of the sets $A$ and $B$:

$$\left\{\begin{array}{llll} P(A_1,B_1) & P(A_1,B_2) & P(A_1,B_3) & \ldots \\ P(A_2,B_1) & P(A_2,B_2) & P(A_2,B_3) & \ldots \\ \ldots & \ldots & \ldots & \ldots \end{array}\right\} \ldots \text{(I}b\text{)}$$

$$\text{abbreviated} = \{P(A,B)\}$$

Similar tables or 'matrices' may be written down for the transition probabilities from the states $A$ to $C$, from $B$ to $C$, and so forth. For reasons of the entropy law of thermodynamics the sets of (I$a$) must all have the same number of members, hence the tables (I$b$) are squares. And they are magic squares, in so far as the sum in any one row and in any one column has the same value, unity, since the $P$'s are probabilities.

Starting from the postulate of continuity which requires a continuous scale of equality values between states we have taken a long stride into the general theory of transitions between states controlled by probabilities; we have thus deduced the background of the quantum theory. Of course, from considerations of such a general character it is not possible to obtain information about the exact magnitudes of the various probabilities $P$ listed in the various $P$-tables; they will depend on a more specific definition of the states $A$ and $B$ in physical terms.

## II. PROBABILITY AMPLITUDES

From the probabilities we now proceed to those probability amplitudes which have been a stumbling block to a common understanding of modern quantum theory and 'are looked upon as a ghost that produces material manifestations' (Lennard-Jones [2]). However, a simple geometrical analogy may lead to a better understanding of this subject.

Consider a triangle $ABC$ and suppose that two of its sides, $P_{AB}$ and $P_{BC}$, are given. These do not determine the length of the third side $P_{AC}$, since the latter depends also on the angle between the two others. Yet although there is no definite general relation between the three sides in a triangle, there certainly is some loose connection between them. In particular, the third side can never be larger than the sum of the two others:

$$P_{AC} \leq P_{AB} + P_{BC} \ldots\ldots\ldots\ldots \text{(II}a\text{)}$$

This triangular inequality determines the metric relation between points $A$, $B$, $C$ only in a loose fashion. For example, the exact relation

$$P_{AC} = (P^2_{AB} + P^2_{BC} - 2P_{AB}P_{BC}\cos\beta)^{\frac{1}{2}}$$

is multivalent because of the free angle $\beta$. When the points are confined to a straight line, then

$$P_{AC} = | P_{AB} \pm P_{BC} |$$

is bivalent; the same holds when the points are on the circumference of a circle. However, with or without such restrictions one can replace the general inequality (II*a*) between quantities $P$ by an equation for less definite quantities $\psi$ in the following fashion. With every length $P_{AB} = P_{BA}$ associate a vector $\psi_{AB} = -\psi_{BA}$ of magnitude $|\psi| = P$ but of free direction. Then adjust the directions in every triangle so that the vectors will satisfy the addition theorem:

$$\psi_{AC} = \psi_{AB} + \psi_{BC} \quad \ldots\ldots\ldots (\mathrm{II}b)$$

This vector equation is self-consistent and general. For example, one can replace the intermediate point $B$ by any other point $D$. But the fact that one can write down and use this vector equation (II*b*) does not give any new insight into geometrical relations which were not already known before; geometers from Pythagoras to Riemann have made their discoveries without explicitly using the vector addition theorem (II*b*). They did not need to, since the latter is implicit in the geometry of distances. Vector rules are elegant formulations, not new geometrical discoveries, and even less do they represent new laws of physics. If it is found that the vector addition rule (II*b*) is always true in regular and irregular, big and small, triangles, this is not a surprising new fact but a matter of definition, since the vectors can be, and always are, adjusted in their directions so that they will satisfy the addition law. Incidentally, if the points $A$, $B$, $C$ . . ., and hence the vectors, are all in one plane, then one can denote the latter by a complex symbol, $\psi = |\psi|e^{i\alpha}$, where $\alpha$ is the directional angle; but this does not imply that vectors are complex entities.

We now return to the transition probabilities $P$ listed in the table $\{P_{AB}\}$ and in similar tables $\{P_{AC}\}$, $\{P_{BC}\}$, and so forth. The probabilities $P$ satisfy the general inequality:

$$P(A_k,B_j) \leqslant 1 \quad \ldots\ldots\ldots\ldots (\mathrm{II}c)$$

and they obey special relations:

$$P(A_k,A_{k'}) = 0 \text{ for } k \neq k' \quad \ldots\ldots (\mathrm{II}d)$$
$$= 1 \text{ for } k = k',$$

characterizing the members of the set $A$ as mutually orthogonal or separable states. The $P$'s also satisfy the magic square rule that the following sums are unity:

$$\Sigma_j P(A_k,B_j) = 1 \text{ and } \Sigma_k P(A_k,B_j) = 1 \;.. (\mathrm{II}e)$$

As with the geometrical problem, one may ask whether there is a general relation or equation between the $P$'s occurring in $\{P_{AB}\}$, $\{P_{BC}\}$, and $\{P_{AC}\}$. This relation must be of such a form that the general summation rule (II*e*) will be satisfied in the table $\{P_{AC}\}$ when it is satisfied in the tables $\{P_{AB}\}$ and $\{P_{BC}\}$. Actually, it is possible to write down a relation between the three $P$-tables which automatically satisfies this condition in the following way.

Associate with every quantity $P$ a vector $\psi$, in this case of magnitude $\sqrt{P}$ and of an indefinite direction $\alpha$ in a plane; two vectors $\psi(A_k,B_j)$ and $\psi(B_j,A_k)$ shall have directional angles $\alpha$ and $-\alpha$ respectively, although the corresponding magnitudes $\sqrt{P}(A_k,B_j)$ and $\sqrt{P}(B_j,A_k)$ are the same. Then adjust the vector directions so that the following multiplication rule is satisfied:

$$\{\psi_{AC}\} = \{\psi_{AB}\} \times \{\psi_{BC}\} \quad \ldots\ldots\ldots (\mathrm{II}f)$$

(compare with the addition rule in the geometrical analogy). The multiplication sign $\times$ denotes a so-called matrix multiplication, (II*f*) being an abbreviation for

$$\psi(A_k,C_m) = \Sigma_j \psi(A_k,B_j)\psi(B_j,C_m) \quad \ldots . (\mathrm{II}g)$$

Since the vectors are all in one plane we use the complex notation $\psi = |\psi|e^{i\alpha}$, and the products on the right of (II*g*) are understood to be carried out according to the rules of complex quantities. But these are details: the essential point is the following. Just as in geometry one can replace the loose connection (II*a*) between three lengths $P$ in a triangle by an equation between vectors the directions of which are adjusted to satisfy this addition law, so one can replace the loose connection between three tables $\{P\}$, expressed by the conditions (II*e*) valid for all three of them, by an equation between vectors whose directions are adjusted so that they will satisfy the matrix multiplication law (II*f*), or (II*g*), the latter being known as the $\psi$-superposition law. But neither the vector addition of geometry nor the matrix multiplication or superposition of probability amplitudes can be considered as a new and independent law of nature. If there are metric relations between the lengths $P$ at all, then to the mathematician the vector addition in a triangle is a matter of course, and to him it is equally a matter of course (not a new independent fact) that sets of entities (I*a*) characterized as orthogonal by the $P$-relations (II*d*) and satisfying the sum rules (II*e*), if interrelated at all, should be interrelated by unitary orthogonal transformations, namely by the $\psi$-equations (II*f*, *g*). The latter are only an elegant formulation of the $P$-metric if one assumes

that there is a general metric of real probabilities at all, i.e. if one believes that Nature is a cosmos rather than a chaos. But due to the habit of representing vectors in a plane by complex symbols, which is commonplace in practical electrical problems, the myth has spread that there is something eerie about those complex quantities $\psi$, which, in spite of their elusive unreality, lead to experimentally confirmed results almost by magic. Thus the student believes that quantum theory is far above the comprehension of the common man —which may be true for the mathematical details but is not true for the underlying ideas.

He will be confirmed in his belief in the incomprehensibility of quantum theory when he is told further that the symmetry principles governing the interaction between identical particles (between two or more electrons, two or more helium atoms, etc.) present us with 'some aspect of great philosophical content beyond the understanding of the physicists . . . who must be content to accept the implications without hoping to penetrate the mystery that is implied': again we quote Lennard-Jones, whose work led to most valuable insights into those very implications. But it is hard to see why it should be beyond the understanding of the physicist that two like particles $a$ and $b$ form a system in which the probability of finding $a$ at $x_1$ and $b$ at $x_2$ is equal to the probability of finding $a$ at $x_2$ and $b$ at $x_1$. It is not cryptic but natural that identical particles should play a symmetrical role when observation is in question. Taking this symmetry of the observable probability for granted, it then follows that the corresponding probability amplitudes must be either symmetric or antisymmetric; that is, the vector $\psi$ must either remain the same or change its direction from $\alpha$ to $-\alpha$ when $a$ at $x_1$ and $b$ at $x_2$ is replaced by $a$ at $x_2$ and $b$ at $x_1$. From this symmetry of the probability $P$ one arrives at such further results as the resonance forces between identical particles, the Pauli exclusion and the Bose accumulation principles (which from this point of view are not principles but consequences), the two quantum statistics, and many other phenomena. The symmetry principles are part of the general theory of probability relations, more general than the $h$-dominated quantum dynamics of conjugate quantities $q$ and $p$.

### III. QUANTUM DYNAMICS

The structural framework of the quantum theory developed in the previous sections does not even mention the quantum constant $h$. The Planck constant occurs only when one proceeds to specifically defined states of real mechanical systems characterized by various co-ordinate values $q$ at various times. Planck's original quantum hypothesis $E = h\nu$ postulated a quantized periodic relationship between energy and time. Later Bohr, Sommerfeld, and Wilson introduced corresponding relations between a space co-ordinate $q$ and its conjugate momentum $p$, thus initiating the era of quantized orbits. However, these quantum conditions were introduced as *ad hoc* hypotheses in order to explain atomic phenomena, and physicists tried hard to detect reasons for this strange periodicity within the framework of classical mechanics. Only when all attempts in this direction had failed did they wisely decide that the time was not ripe for an explanation of the quantum rules, and that one should accept them as axiomatic. But this attitude, which was reasonable as a temporary expedient, hardened into the creed that the quantum rules themselves are fundamental, are irreducible to anything more elementary, and not even in need of further justification. Then in 1926 new and better rules were found, viz. the Schrödinger operator rule, $p = (h/2i\pi)\mathrm{d}/\mathrm{d}q$, and the Born commutation rule, $qp - pq = h/2i\pi$. These rules again postulate a periodic relationship between co-ordinates and their conjugate momenta, though in a form more intricate than the older prescriptions of Planck and Bohr. But this periodicity is still the chief enigma of the quantum theory; every student is puzzled by it, and only when the experts tell him that there is no further explanation—nor even the need for an explanation—does he slowly resign himself to accept the quantum rules of periodicity as a matter of fact. Yet there is no necessity for this attitude of resignation. Instead of introducing the quantum rules of Born and Schrödinger as new and independent assumptions, supplementing the general $\psi$-superposition rule (II$g$)—that is, instead of introducing the wave mechanics of Schrödinger or the matrix mechanics of Born and Heisenberg by decree on grounds of microphysical evidence—one can show that the quantum postulates of Born and Schrödinger, with their periodicity, are mathematical consequences of the general $\psi$-superposition (probability metric) applied to the special case of quantities $q$ and $p$ as conjugates, if conjugacy is defined in the following manner:

Imagine an assemblage of many independent particles, all in the same external force field. The original position and momentum values $q_0$ and $p_0$ at time $t_0$ may vary from particle to particle, but

all initial $qp$-values represent points within a certain volume range $V_0$ of the whole $qp$-space, or 'phase space.' In the course of time the various $qp$-points will move to other places in phase space where they will occupy other volumes $V$ at other times $t$. The classical equations of motion now yield the simple yet important result that the volumes $V$ occupied at various times $t$ will all have the same magnitude, or $V = V_0 =$ constant. This is the well known Liouville theorem of classical mechanics. Statistically, over a long time interval, the probability of a member of an assemblage of particles residing in $V_0$ is as large as that of residing in $V$ when $V = V_0$. In short, there is a constant probability density in $qp$-space. This result of classical statistical mechanics is very general; it holds not only for particles, but also for systems of particles, atoms, molecules, and mechanical systems in general. It is a statistical result, though resting on the deterministic equations of motion.

Now, although deterministic mechanics must be abandoned in favour of a statistical theory (for the sake of cause-effect continuity, section I), classical mechanics is not quite wrong. It is in every way most plausible, in these circumstances, that the one feature of classical mechanics to be preserved in the new statistical theory should be the statistical result of the constant probability density in $qp$-phase space. That is, in classical as well as in the new mechanics, two quantities $q$ and $p$ can be defined as mutually conjugate when they possess a constant probability density in $qp$-space. To this must be added the familiar fact that only co-ordinate differences and momentum differences rather than absolute $q$ and $p$ values, have a physical significance.

When the $\psi$-multiplication rule of section II is applied to conjugates $q$ and $p$ as defined above, then it becomes a mathematical necessity [4] that the vector $\psi(q,p)$ must have a constant magnitude for all values of $q$ and $p$, and that its directional angle must change in proportion to $q$ for given $p$, and in proportion to $p$ for given $q$. After a change of 360° the original vector $\psi$ is encountered again; that is, $\psi$ is periodic in $q$ as well as in $p$. When the vector $\psi$ is written in complex notation the same result reads:

$$\psi(q,p) = \exp\,(2i\pi qp/\text{constant})\ldots.(\text{III}a)$$

It is the simplest case of a Schrödinger wave function. The constant denominator is usually denoted by the now familiar letter $h$.

The reader will notice that neither the constant probability density in $qp$-space nor the $\psi$-multiplication or superposition rule assume any periodicity. Only when taken together do they require $q$ and $p$ to occur in the periodic probability relation (III$a$) with wavelength $\lambda = h/p$. The strange quantum periodicity is thereby explained, that is, reduced to a simple and almost common-sense non-quantal background, rather than ascribed to principles of duality and complementarity of cryptic origin. Of course, deciding what is strange and what is of a common-sense character is a matter of personal judgment. It is after all very simple to write down formulas such as the Schrödinger operator rule or the Born commutation rule—so simple indeed that many have forgotten that the periodicity of quantum theory in itself is of a problematic character. The present article has tried to show that quantum theory can indeed be understood as a consequence of the non-quantal principle of cause-effect continuity which leads first to the general probability metric of sections I and II, and yields, when applied to conjugate co-ordinates and momenta, the periodic probability relation between conjugates which is the most essential feature of the quantum theory.

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# Insect song

J. W. S. PRINGLE

The sounds made by different species of insects, or by members of the same species in different moods, are characterized by differences in the time distribution of pulses of sound rather than by differences in pitch or tone quality as with birds and mammals. The inability of the human ear to distinguish this significant feature has long obscured the rich variety of insect songs, but modern recording devices have made it possible to analyse them in detail.

The Sinhalese name for the cicada is *Răhaiya*, meaning 'a hundred nights', and is derived from the belief that the insects sing for that period of time and then burst. The villager will point out on the trunks of trees cicadas that have suffered this fate; they are, in fact, the split nymphal skins from which the adult has emerged after several years of subterranean existence. All over the world there is a similar picturesque legendry wherever there are insects that have forced themselves on the notice of man by their capacity to produce sounds.

The most elaborate patterns of song and the most complicated organs for sound-production are concerned with mating behaviour. Provided that the other sex has sense organs capable of receiving the sound and of determining the direction of its source, a means is provided by which insects in the correct physiological condition can be brought into close proximity; other, usually visual, stimuli can then assume control of the movements of orientation and coition. As with all other animals except man, sound-production is thus an indication of mood—of that combination of internal states which renders the insect ready for a particular behavioural act; the abstract symbolism characteristic of human speech [8] is, so far as is known, absent in insect songs. Moods other than sexual preparedness may be associated with sound-production. This applies especially to alarm, when the noise may deter a predator or warn other members of the species, but in insects, as in birds [4], the sounds produced under these conditions tend to be simpler and less specific than those concerned with mating.

In recent years much progress has been made towards an understanding not only of the role that sounds play in the behaviour of insects, but also of the physical and physiological mechanisms involved in sound-production. In many respects these differ fundamentally from the method used by the other great group of land animals, the Vertebrates. Insects have no respiratory airstream capable of inducing passive vibration in structures like the vocal cords, whose operation is analogous to that of the reed of a clarinet. Their hard exoskeleton and internal tracheal air spaces, however, provide an ideal sounding-board, capable of efficient transformation of vibrational energy into airborne sound. It is possible that this provides the key to an understanding of the many strange freaks of structural evolution found in insects, for vibrations are readily produced whenever two parts of the body come in contact, or by buckling of the skeleton due to the contraction of muscles attached to it. Both in cicadas and in several groups of the Orthoptera, comparative studies can suggest a probable course of evolution of the sound-producing habit through the addition of a resonator to a vibration-generating mechanism.

Figure 1 shows examples of some well-known insect singers. Numbers 1–4, 7, and 9 are Orthoptera; 5 and 6 are cicadas (Homoptera); 10 and 11 are beetles (Coleoptera); and 8 is a Pentatomid bug (Heteroptera). All except the cicadas produce sound by the rubbing of one part of the body on another (stridulation), one part often having a ribbed or toothed file which increases the contact resistance. Cicadas utilize the buckling of a specially modified region of the skeleton on the dorsolateral wall of the first abdominal segment; this is effected by means of a large pair of muscles inserted

FIGURE 1 (*see facing page*) – *Some insects which produce sounds.* (*Natural size*). (1) Anaedopoda lamellata (*L*), *Tanganyika.* (2) Oedopoda germanica (*Latr.*), *Europe.* (3) Scyllina picta (*Bruner*), *S. America.* (4) Pneumora variolosa (*L.*), *S. Africa.* (5) Zammara smaragdina (*Walk.*), *Mexico.* (6) Thorpha sessiliba (*Dist.*), *Australia.* (7) Brachytrypes portentosus (*Licht.*), *India.* (8) Catacanthus violarius (*Stål.*), *New Guinea.* (9) Gryllotalpa gryllotalpa (*L.*), *Europe.* (10) Geotrupes stercorarius (*L.*), *Britain.* (11) Aromia moschata (*L.*), *Britain.*


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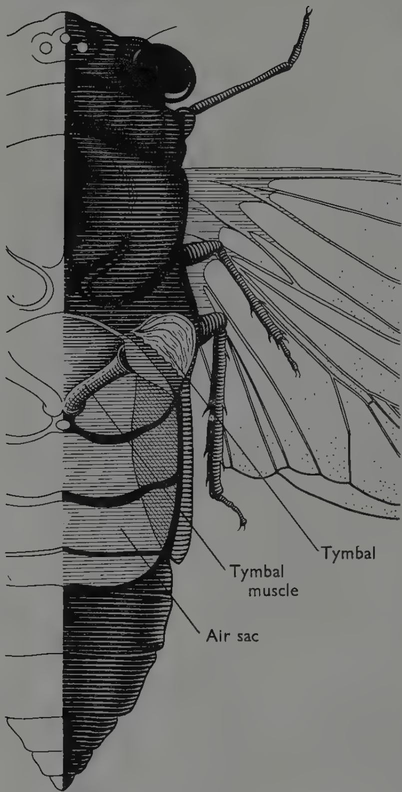
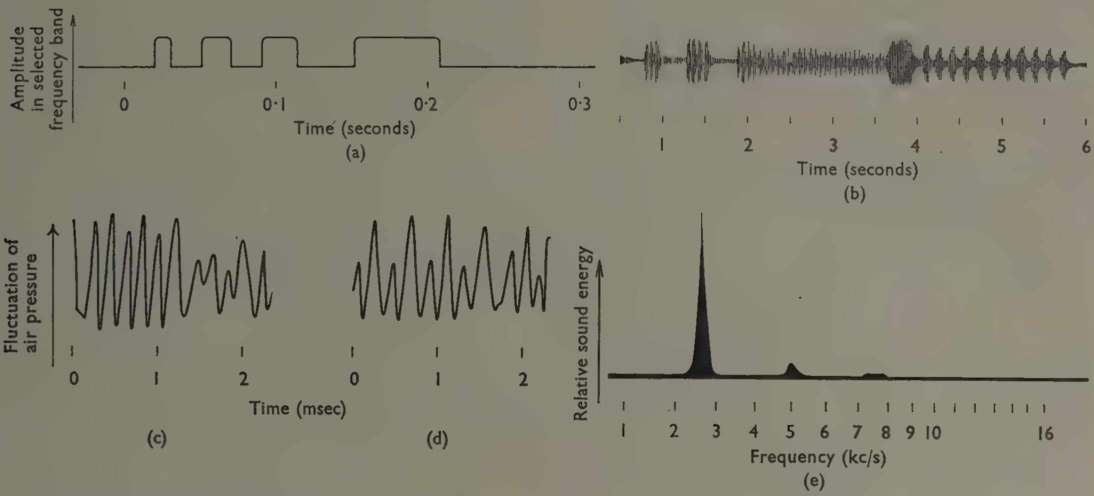



FIGURE 2 – *The sound-producing organs of a cicada* (Platylomia abdulla (*Dist.*)) *seen through a window cut in the abdominal dorsum; the tymbal cover has been removed.*


FIGURE 3 – *Photomicrograph of the abdominal stridule of* Prestalia granulata (*Stål.*) (*Pneumoridae*).

by a slender apodeme in one corner of the 'tymbal' (figure 2). The structure can be seen in *Zammara smaragdina* (figure 1 (5)), but more usually, as in *Thorpha sessiliba* (figure 1 (6)), it is covered by a protective fold of cuticle. The whole of the abdomen of the male cicada—only the male sings—is occupied by a large tracheal air space, which is tuned to the frequency of tymbal vibration when the abdomen is expanded by special accessory muscles; another accessory muscle can increase the curvature of the tymbal and so increase the intensity of the click produced when the surface buckles [6].

The file-and-peg method has many variations. In the crickets (Gryllidae, figure 1 (7 and 9)) the file is on the under-surface of the right wing and there is a peg on the left wing; the bases of both wings are strengthened to form a resonating membrane. In *Anaedopoda lamellata* (Tettigoniidae, figure 1 (1)) the file is on the under-surface of the left wing, and the right wing alone has a special vibrating membrane at its base. In the Acrididae the hind-legs are usually involved; *Oedopoda germanica* (Oedopodinae, figure 1 (2)) has the file on the wing, whereas in *Scyllina picta* (Truxalinae, figure 1 (3)) it is on the hind femur. In the male of *Pneumora variolosa* (Pneumoridae, figure 1 (4)) the hind-legs are again used, but the file is on the abdomen (figure 3) and there are large abdominal air-sacs analogous to those of cicadas. In the Coleoptera, sound may be produced by movement of the prothorax on the mesothorax (*Aromia moschata*, Cerambycidae, figure 1 (11)); by movement of the hind coxae (*Geotrupes stercorarius*, Scarabaeidae, figure 1 (10)); or by rubbing the tip of the abdomen on the elytra, as in the little water-beetle *Hygrobia hermanni* (Peloriidae), in which the sound can be made even under water. Sometimes



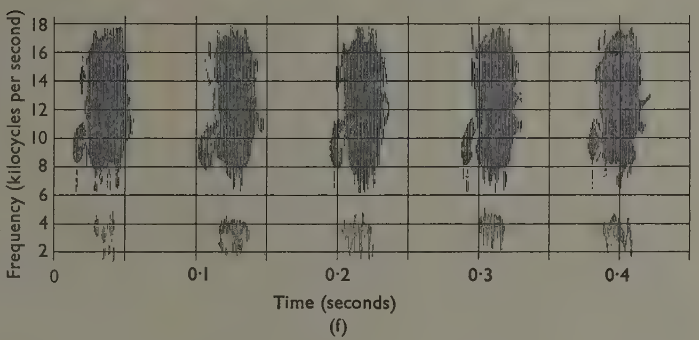




FIGURE 4 – *Insect songs as displayed by a variety of recording instruments.* (a) *Tuned amplifier.* Amblycorypha oblongifolia (*De Geer*) (*Tettigoniidae*) [3]. (b) *Oscillograph.* Terpnosia ridens (*Pringle*) (*Cicadidae*) [6]. (c, d) *High-speed oscillograph.* Terpnosia stipata (*Walk.*) (*Cicadidae*) [6]. (e) *Frequency analyser.* Oecanthus pellucens (*Scop.*) (*Gryllidae*) [1]. (f) *Sound spectrograph.* Neoconocephalus ensiger (*Harris*) (*Tettigoniidae*) [2].

the larvae also have stridulating organs and on capture produce a vibration which may well deter a foraging bird. There is an almost equal variety in the Hemiptera.

Not only are the methods of producing sound in insects different from those found in the Vertebrates, but the quality also is very different. This has been obvious to naturalists from the earliest times, but it is only with the use of modern recording methods that the characteristics have been fully elucidated. The small size of insects limits the frequency to which structures in their bodies can resonate, and high, even supersonic, frequencies are the rule (range 2·5 to 50 kc/s). The sound also tends to be broken up into a series of very short pulses at a frequency of 20 to 500 per sec., a feature which may be correlated with the fundamentally different physiology of the auditory organs. Whereas the ears of Vertebrates, at least in all the higher land-dwelling members of the sub-phylum, contain groups of cochlear nerve endings each sensitive to a slightly different frequency band and so conferring an ability to discriminate the pitch of a received sound, insect ears are either untuned or have a single overall band of frequency coverage and give a pattern of impulses in the auditory nerve which follows the modulation or amplitude envelope [7]. Sound to an insect thus has the function of a carrier wave, analogous to the radio-frequency oscillation in wireless telegraphy, and the information which is characteristic of the species or the mood is carried in the pattern of pulses rather than in pitch variations or tone quality as with birds and mammals. It is because of the inherent inability of the human ear to detect the significant features of many insect songs that the variety existing in nature has long gone unsuspected.

Four main types of recording instrument are now available which can be used in the study of insect songs, and the records which these give are illustrated in figure 4. The time-pattern can be determined with a tuned amplifier; figure 4(*a*) represents a single chirp in the song of an American

katydid (Orthoptera, Tettigoniidae). A more rapid record would show each of these sounds to be composed of a series of pulses corresponding to the disengagement of one tooth on the file from the peg on the other wing; the chirp thus involves four movements of the wings. The frequency of emitted sound is not displayed by this method, but may be determined approximately from the tuning of the amplifier.

If the microphone is connected to a wide-band amplifier, a maximum of information about the song is obtained from the oscillogram. Figure 4 shows such an analysis of the song of a cicada. The pulses corresponding to the clicking of the tymbal occur in groups with a complicated rhythm; the 'phrase', which is characteristic of the species, is shown in figure 4(*b*). Fast oscillograms of the sound waveform within a single pulse (figure 4(*c*), (*d*)) indicate that the tymbal has several modes of vibration, chiefly at 2250 c/s and at the second harmonic, 4500 c/s, which is the resonant frequency of the abdominal air-sacs.

The third method, which is conveniently used as a complement to oscillography, is frequency analysis. Figure 4(*e*) shows the song of a cricket (Orthoptera, Gryllidae); insects of this family emit a musical tone purer than those of any other, the sound-frequency being determined by the rate of passage of the wing-file teeth over the peg, so that one pulse of sound is produced by an entire wing-stroke. The harmonic content of this song is very low, and the oscillogram of each pulse is a pure sine-wave. The frequency-analyser does not display the time-pattern of pulses in the song and, by itself, contributes little to an understanding of the features which are of significance to other insects.

The most refined apparatus is the sound-spectrograph or Vibranalyzer. This involves the making of a faithful magnetic recording of the song using a microphone, amplifier, and recording speed capable of noting the highest frequencies, and then analysing a loop by means of which the chosen phrase may be repeated indefinitely. The record made by the instrument (figure 4(*f*)) shows the time-pattern and frequency along the X and Y axes respectively, the intensity at any given frequency being indicated by the density of the print. A choice between maximum frequency resolution and maximum time resolution can be made by changing the bandwidth of the analysing filter. The song shown in figure 4(*f*) is that of a small Tettigoniid grasshopper; each note corresponds to one complete wing movement and is composed of a succession of rapid pulses corresponding to the number of file-teeth used. An appreciable fraction of the total sound energy is in the supersonic range, and the wide band of frequencies emitted gives the song a hissing quality to the human ear.

The file-and-peg method of sound-production necessitates elaborate structural adaptations but demands no major modification of physiology. The high frequencies in the song are produced by the resonance of membranes and by the use of serrations; the movement itself is not more rapid or more frequent than other movements made by insects. This is not the case with the tymbal system of the cicadas and their relatives among the Homoptera. In some cicadas the pulse frequency, which corresponds to the movements of the tymbal, may be as high as 400 c/s. In order to achieve this speed of contraction and relaxation the tymbal muscle has evolved a form of activity-cycle which is different from that of all other striated muscles except the indirect wing muscles of flies, bees, and certain other insects [5]. The rhythm of contraction is determined, not by the central nervous system but in the muscle itself, by a mechanism involving a deactivation of the contractile fibrils by the mechanical stimulus of the tymbal movement. The detailed nature of this process is not yet clear; its elucidation is perhaps the most important problem facing the zoologist in his search for an understanding of the physiology of insect song.

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# The gas-liquid chromatogram

A. T. JAMES

The great importance of chromatography needs no emphasis. The recent development of a new form of the method, depending upon selective distribution of substances between gaseous and liquid phases, has still further extended its range. Although applicable only to volatile substances, gas-liquid chromatography has a scope as wide as that of the various forms of distillation, which it closely resembles, since it can be applied to substances exerting a partial vapour pressure in the column as low as 1 mm of mercury.

In the course of time distillation has evolved into an elaborate technique for fractionating mixtures of substances having different volatilities. In essence the technique of fractional distillation is simple enough. Vapours derived from the boiling mixture are passed up a vertical column against a falling stream of condensed vapour, and the different components of the mixture distribute themselves between the vapour and liquid streams according to their relative volatility, the most volatile tending to concentrate at the top of the column and the least volatile at the bottom. To obtain the most effective separation it is necessary to pass the vapour and liquid phases through as many equilibrium stages or theoretical plates as possible, the latter being defined as that length of column in which vapour leaving the top is in equilibrium with liquid leaving the bottom. To attain this end, various devices are inserted into the column to promote the most intimate contact between the vapour and liquid streams. The smaller the length of the theoretical plate the more plates there are in a given length of column, and the easier it becomes to obtain pure fractions. The best laboratory fractionating columns may have efficiencies as high as 200 plates, but large-scale industrial columns are much less efficient.

Because the fractionating column is filled with the mixture to be separated, a substance leaving the column is followed immediately by the next; in consequence the fractions always overlap, the extent of the overlap depending on the efficiency of the column. From the industrial point of view this is no great defect, since the recycling of such fractions is easy. From the analytical point of view, however, it is much more serious, since a 100 per cent yield of any one component is impossible. An additional disadvantage for the analyst is that it is difficult to make a high-efficiency fractionating column capable of handling quantities of material less than 5–10 g. Furthermore, as the columns need to be run slowly, it takes a long time to obtain information about the number of components in a complex mixture.

One variant of distillation technique, known as carrier distillation, is to add to the mixture a chemically inert substance having a boiling point such that one component can be selectively removed in the distillate. This process has lately been modified in a novel manner by replacing the vapour of the carrier substance by a permanent gas and the liquid phase of the column by a non-volatile liquid. This non-volatile liquid is distributed over the surface of an inert porous solid, usually kieselguhr, that is packed into the column. The porous nature of this packing allows the permanent gas stream to be passed rapidly through the column with the aid of reasonably low excess pressures; the pressures used normally lie between 5 and 80 cm of mercury above atmospheric pressure. When a mixture of volatile substances is applied to the top of the column and is blown down by the gas stream it will separate into its components, in order of their relative volatility, in the stationary liquid phase. Provided the column is long enough, the substances emerge quite separately in the stream of gas which emerges from the end of the column. Such columns, with efficiencies of 1000–2000 theoretical plates, are easily constructed.

This system is in effect a form of chromatography, a gas-liquid form essentially similar in behaviour to the liquid-liquid (or partition) chromatography invented by A. J. P. Martin and R. L. M. Synge in 1941. It can be considered as the last form of chromatography to be applied, since now all the readily obtainable combinations of solid, liquid, and gaseous phases—liquid-solid, gas-solid, liquid-liquid, and gas-liquid—have been exploited. Martin and Synge suggested the feasibility of the gas-liquid chromatogram in their original paper on the liquid-liquid chromatogram,

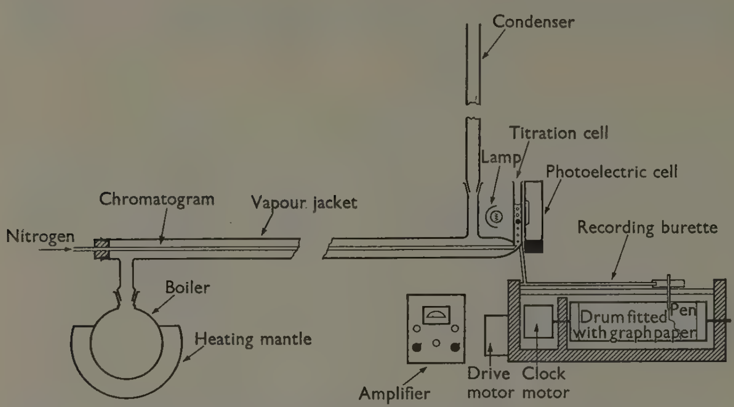



FIGURE 1 – *Schematic diagram of apparatus for gas-liquid chromatography, using automatic titration to detect volatile acids or bases emerging from the column.*

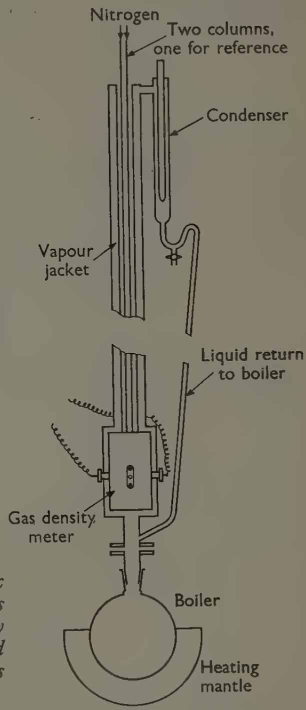



FIGURE 2 – *Schematic diagram of apparatus using automatic density measurement to record the presence of vapours leaving the column.*

but it was not developed until ten years later [1]. The subject has been reviewed elsewhere [11–13] more fully than is possible here.

The use of a gas as the mobile phase of a chromatogram confers a number of advantages. (1) The high rate of diffusion in the gas allows equilibrium with the liquid phase to be attained rapidly; (2) the compressibility of a gas allows the use of long thin columns even at rapid rates of flow; (3) it is easier to detect vapours in permanent gases than solutes in solvents, particularly in the case of such unreactive substances as saturated hydrocarbons; and (4) high column efficiencies are attainable even at rapid rates of flow, so that precise separations can be carried out more rapidly than by any other technique.

On an analytical scale the apparatus is simple. It consists of a length of narrow glass or metal tubing packed with a mixture of the stationary phase and kieselguhr, maintained at a constant temperature by a vapour jacket. A source of nitrogen of constant pressure [1, 2] is applied to the column, the effluent from which is passed into a detector which records automatically the amount of vapour present in the gas stream.

Three methods of detection have so far been widely used: the first records the total mass of material (or some function of the mass) that has emerged, and the second and third record the concentration at which material is emerging.

A total-mass record—such as is produced by automatic titration (figure 1) of volatile acids or bases [1], or by volume measurement after removal of carrier gas [3]—will show a series of steps, representing the integrals of the more familiar peaks seen in chromatograms; some examples are shown in figures 3–5. Instruments which measure the concentration at which material is emerging from the chromatogram, such as catharometers [4–6] or the gas-density meter [8], show peaks on the final chart, and examples of their use are shown in figures 6–8. A typical apparatus is shown diagrammatically in figure 2.

### THE ADVANTAGES OF THE GAS-LIQUID CHROMATOGRAM

As pointed out, these columns can be operated at high efficiencies even with rapid rates of gas-flow, so that separations can be carried out quickly. Figure 4 shows a separation of mono-, di-, and tri-methylamines completed in eight minutes, the record showing the relative amounts of each component. Analyses of more complex mixtures are shown in the other figures.

The high sensitivity of the technique allows the use of very small samples. Furthermore, the material leaving the apparatus can be recovered and subjected to further chemical study if required. The range of materials that can be handled is the same as that amenable to distillation, even at pressures down to 1 mm of mercury.

The apparatus can be made fully automatic, so that many analyses can be carried out with the

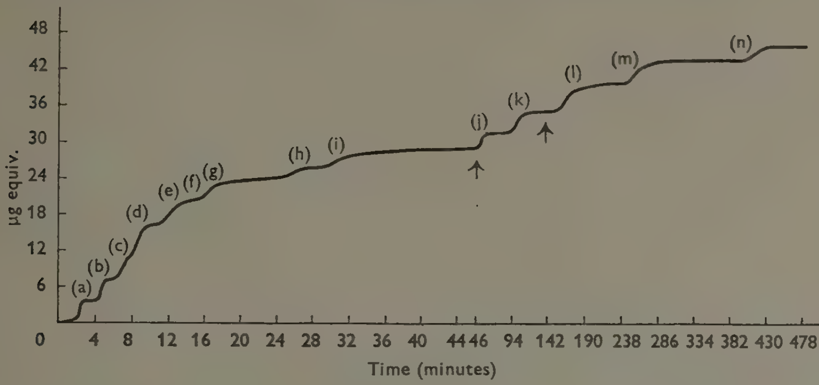
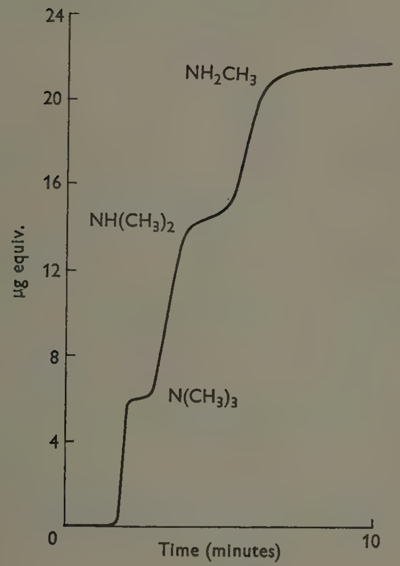



FIGURE 3 – *Separation of fourteen fatty acids ranging in chain length from* $C_2$ *to* $C_{11}$*; at the first arrow the time scale was shortened; at the time indicated by the second arrow the rate of flow of nitrogen was increased.* (a) *Acetic acid,* (b) *propionic acid,* (c) iso*butyric acid,* (d) n-*butyric acid,* (e) iso*valeric acid,* (f) *α-methylbutyric acid,* (g) n-*valeric acid,* (h) *γ-methylvaleric acid,* (i) *hexanoic acid,* (j) *heptanoic acid,* (k) *octanoic acid,* (l) *nonanoic acid,* (m) *decanoic acid,* (n) *hendecanoic acid.*



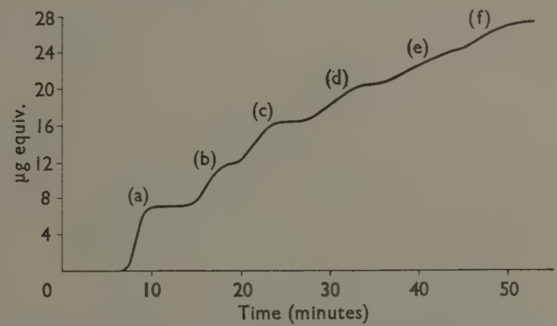




FIGURE 5 (*above*) – *Separation of aromatic amines, using automatic titration in glacial acetic acid for their detection. Column temperature* 137° *C; stationary-phase liquid paraffin.* (a) *Aniline,* (b) *methylaniline,* (c) *dimethylaniline,* (d) m-4-*xylidine,* (e) *dimethyl-*o-*toluidine,* (f) *diethylaniline.*

FIGURE 4 (*left*) – *Separation of mono-, di-, and tri-methylamines as recorded by the automatic burette. Column temperature* 78·6° *C; stationary phase glycerol.*

minimum of fatigue. The columns are easily and quickly packed and can be used many hundreds of times before exhaustion, this occurring only by stripping of the stationary phase. Results are highly reproducible; provided temperature and nitrogen flow are kept constant, a given substance will always emerge at the same time.

## SOME APPLICATIONS OF THE TECHNIQUE

The introduction of a new separation technique invariably results in the discovery of substances previously undetectable: compare, for example, the discovery of new amino acids, nucleotides, purine bases, etc., by the use of paper and ion-exchange chromatography. Gas-liquid chromatography has proved to be no exception. For example, a study of naturally occurring fatty acids by the author has shown the presence of not only the odd-number fatty acids, the existence of which was long questioned (see also the work of Shorland and his co-workers [14]), but also odd-number unsaturated acids and at least two types of branched-chain fatty acids. These acids occur in significant amounts in sources as widely different as human sebum [15] and goat milk fat [16]. In biological research the high sensitivity of the technique makes it ideal not only for elucidation of the structures of new compounds but also for studying their metabolism.

The elucidation of the structure of reaction by-products, or of substances of biological origin obtainable only in small amount, is difficult by conventional chemical methods. Where chemical degradation can give rise to volatile fragments, the

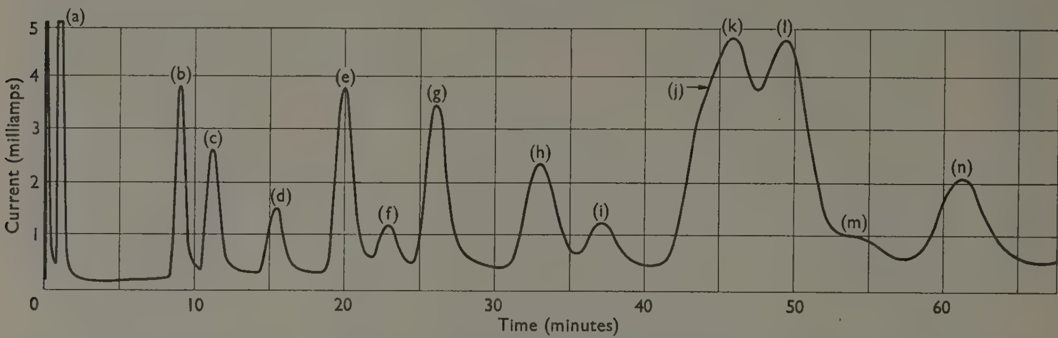



FIGURE 6 – *Separation of isomeric aliphatic hydrocarbons, using gas-density measurement to identify the zones leaving the column. The ordinate represents the reading of the recording galvanometer in this figure and in figures 7 and 8.* (a) *Air peak,* (b) iso*pentane,* (c) n-*pentane,* (d) *2:2-dimethylbutane,* (e) cyclo*pentane plus 2-methylpentane and 2:3-dimethylbutane,* (f) *3-methylpentane,* (g) n-*hexane,* (h) *methyl*cyclo*pentane, 2:2-dimethylpentane, 2:4-dimethylpentane,* (i) *2:2:3-trimethylbutane,* (j) *3:3-dimethylpentane,* (k) *2-methylhexane,* (l) cyclo*hexane plus 2:3-dimethylpentane and 3-methylhexane,* (m) *3-ethylpentane,* (n) n-*hexane.*

gas chromatogram can provide the necessary information as to their structure and quantity. For example, the nature of the side-chains attached to the 4-hydroxyquinoline-N-oxide nucleus of a naturally occurring streptomycin antagonist was established by running through a column oxidation products derived from only 10 mg of the impure substance [17].

In physicochemical studies the gas chromatogram can provide information on latent heats of evaporation, free energies of solution, and so on, and on the effect on such parameters of small changes in structure. The highly purified substances required for conventional techniques are not necessary, since the impurities can usually be removed in the course of the measurement.

The high resolving power of the technique has frequently had the result of demonstrating the impurity of substances formerly thought pure (figure 7) and has obvious uses in the examination of volatile intermediates for synthetic work. The consequent simplicity of determining the purity of chemicals supplied for laboratory use is likely to make organic chemists much more critical of the quality of commercially supplied reagents.

For routine examination of the composition of feed stocks and volatile commercial intermediates and final products, gas-liquid chromatography has no equal. Its ability to deal with complex mixtures has advantages over even the mass spectrometer, since when applied to mixtures the latter produces results in a form requiring great mathematical ingenuity for their understanding; the method should not, however, be considered as a rival to either the mass spectrometer or light-absorption techniques, since both these can give more useful information, when dealing with single substances, than the gas chromatogram can. The apparatus can be made robust enough for plant use, and the long life of the columns and the reproducibility of results make it suitable for operation by workers with relatively little training in its use.

## FACTORS GOVERNING COLUMN EFFICIENCY

The main factors effecting column efficiency are rate of flow of gas, evenness of column packing, temperature, and nature of the stationary phase. Secondary factors such as the dead volume of the detector and the method of loading the columns will not be dealt with here (see [9]).

The rate of diffusion of the vapours in the stationary phase of the column seems to be the controlling factor in efficiency. Measurement of the

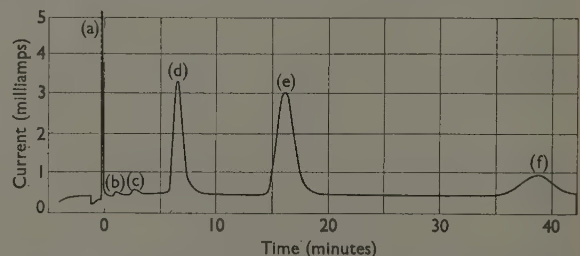



FIGURE 7 – *Analysis of a commercial sample of methyl myristate. Column temperature 197° C; stationary phase Apiezon M vacuum stopcock grease.* (a) *Air peak,* (b) *methyl* n-*octanoate,* (c) *methyl* n-*decanoate,* (d) *methyl* n-*dodecanoate,* (e) *methyl* n-*tetradecanoate,* (f) *methyl* n-*hexadecanoate.*

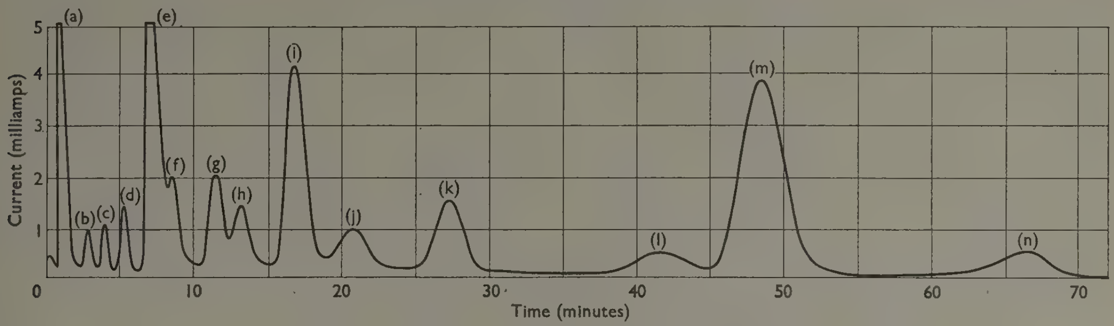



FIGURE 8 – *Separation of methyl esters of a variety of straight- and branched-chain fatty acids. Column temperature* 197° *C; stationary phase a high-boiling lubricating-oil extract.* (a) *Air peak,* (b) *methyl* n-*pentanoate,* (c) *methyl* n-*hexanoate,* (d) *anti*-iso*heptanoate,* (e) *methyl* isooctanoate, (f) *methyl* n-*octanoate,* (g) *methyl anti*-isononanoate, (h) *methyl* n-*nonanoate,* (i) *methyl* iso*decanoate,* (j) *methyl* n-*decanoate,* (k) *methyl anti*-isoundecanoate, (l) *methyl* isododecanoate, (m) *methyl* n-*dodecanoate,* (n) *methyl anti*-isotridecanoate.

band-width of a series of zones shows the efficiency to increase with time of emergence from the columns. Clearly, increasing the flow-rate of nitrogen decreases the time of contact with the stationary phase and so decreases the efficiency. For the greatest efficiency columns are run as slowly as circumstances permit.

Highly viscous stationary phases, with their attendant slow diffusion rates, will give relatively inefficient columns, so that there is an advantage in using as mobile a stationary phase as is compatible with the need for low volatility at the operating temperature. Increase of column temperature increases column efficiency, owing to the increase in the diffusion rates in the liquid phase, but decreases separation factors.

### CHEMICAL STRUCTURE AND CHROMATOGRAPHIC BEHAVIOUR

The interpretation of the behaviour of different substances in liquid-solid and liquid-liquid chromatograms is rendered difficult by interactions between the two phases. With the gas-liquid chromatogram, however, these are relatively minor effects, and in many cases it is possible to ascribe relative movements down the columns to differences in solute-solvent interaction.

In columns with stationary phases consisting of paraffin hydrocarbons the solute-solvent interaction is of the Van der Waals type only, and depends on the number, nature, and configuration of the atoms in the molecule. This is demonstrated in figure 9, which shows the relationship between molecular weight and configuration and the logarithm of the time of emergence from the column (for convenience measured relative to a standard substance) for a variety of saturated hydrocarbons; the stationary phase consisted of *n*-octadecane at 65° C. For a homologous series a linear relationship exists between the logarithm of the time of emergence and the chain length, since increasing the chain length by one —$CH_2$— group increases the free energy of solution by a constant amount

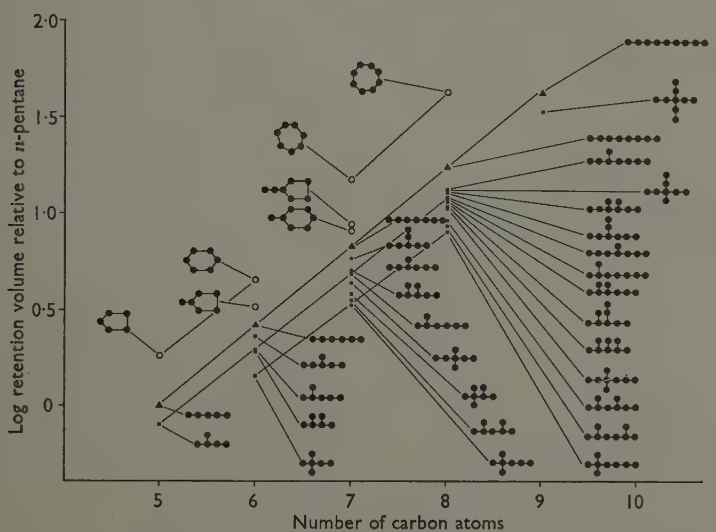



FIGURE 9 – *Relationship between structure and rate of movement down the column (relative to* n-*pentane) of a variety of saturated hydrocarbons. Column temperature* 65° *C; stationary phase* n-*octadecane.*

(approximately 750 cal/g mole under these conditions). This linear relationship conveniently allows the time of emergence of any member in a series to be calculated from knowledge of the behaviour of two or three members. It is also a great aid in the identification of substances of unknown structure.

It should be noted that closeness of boiling point is not reliable as a means of judging the feasibility of a given separation, since interaction forces in a pure liquid are not necessarily the same as those occurring in the stationary phase of a column. With substances of similar chemical structure, such as isomeric saturated hydrocarbons, closeness of boiling point does indicate that the substances will run close together in the gas chromatograms (see figure 9). Substances of identical boiling point but different chemical structure are, however, much easier to separate, since a stationary phase can often be chosen which has a greater energy of interaction with one component than with the other.

In stationary phases other than paraffin hydrocarbons, different forces come into play due to the interaction of polar groupings. For example, a polyether with the repeating unit —$CH_2CH_2$—O— will show hydrogen bonding with substances possessing an active hydrogen atom, such as chloroform, primary and secondary amines, alcohols, phenols, etc.; with substances containing none of these groups, however, only the weaker Van der Waals forces will be involved. Aromatic hydrocarbons show strong interactions with molecules containing the groups —$NH_2$, —OH, —$COCH_3$, —CN, —$NO_2$, the interaction energy increasing in the order shown.

For a given separation the question to be asked is what the differences are in structure between the substances involved. Differences in the number and nature of alkyl groups would indicate that separation will be based on differences in Van der Waals interactions, requiring a fatty stationary phase. Possession of groups of different polarity will indicate the need for a stationary phase capable of interacting with such groups.

It is not necessary to maintain the column at a temperature comparable to the boiling point of the materials under examination, since many substances will move reasonably rapidly down the column when these partial pressures are of only a few millimetres. It is always advantageous to run the column at as low a temperature as possible in order to obtain large separation factors. For example, the —$CH_2$— separation factor (the time of emergence of a substance relative to the time of emergence of the next lowest homologue) in stationary phases consisting of paraffin is 3·5 at 23° C but only 1·56 at 200° C.

Mixed stationary phases may be used so that the relative contribution of Van der Waals and polar solution forces are different from those produced in either pure solvent. In this way it is possible to make the stationary phase specifically hold back one component of a mixture. This additional degree of freedom makes available many more stationary phases than was previously thought possible.

Most of the work so far reported has been devoted to analytical problems, but the technique can also be used for the preparation of pure substances in quantities of 50–100 g. J. C. Tatlow [10] has used the gas-liquid chromatogram for the preparation of pure fluorocarbons in 10 g quantities; no other purification technique could produce pure compounds from the crude reaction products.

ACKNOWLEDGMENT

Thanks are due to the Editors of the *British Medical Bulletin* for permission to reproduce diagrams already published.

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# Sea-urchin spermatozoa

LORD ROTHSCHILD

The investigation of a biological phenomenon usually starts with a study of its morphology. This article is mainly concerned with the morphology of sea-urchin spermatozoa up to the time of fertilization, a subject which has lately been greatly facilitated by electron microscopy. Now that we can cut sections as thin as 250 Ångströms, structures can be seen whose existence was previously unknown. In addition, inferences can be made about the functions of these structures from other information about the biological phenomena under investigation.

Fertilization has been more intensively studied in sea-urchins than in any other animal or plant, for the following reasons:

1. Sea-urchin eggs and spermatozoa can be obtained in astronomical numbers. Some idea of the number of spermatozoa produced by a sea-urchin may be obtained from figure 4, which is a photograph of the common British sea-urchin, *Echinus esculentus*, spawning under water. The streams of semen being ejected from the five genital apertures are about 4½ cm long and contain some 100 000 000 spermatozoa. One sea-urchin can produce 1000 cm of semen during one breeding season.
2. Sea-urchin eggs are transparent, and therefore ideal material in which to observe under the microscope what happens when the sperm enters the egg.
3. Fertilization is easy to achieve *in vitro*.

Examination of a live or stained sea-urchin spermatozoon under the light microscope reveals that it consists of a pear-shaped head some 3½ μ long and 1 μ wide at its base, a globular mid-piece, and a vibratile tail which is about 0·2 μ in width and 50 μ long. Figure 1 is a reproduction of drawings published by the great microscopist G. Retzius in 1910 [11]. Apart from additional observations made possible by the electron microscope and those of G. T. Popa [10], who appeared to be blessed with second, though somewhat erratic, sight, Retzius's drawings might have been done in 1956 and not in 1910. The small refractile structures which can be seen at the tips of the heads in figure 1 do exist, though they are invisible in the living spermatozoon, even with the highest phase-contrast magnification.

### THE TAIL AND ITS MOVEMENT

Sea-urchin spermatozoa swim by propagating S-shaped deformations, or waves of bending, along their tails, in an antero-posterior direction. The frequency of propagation is 30–40 waves per second. The remarkable photomicrograph reproduced in figure 9(*a*), which was taken by Sir James Gray of Cambridge University and M. M. Swann of Edinburgh University, shows these spermatozoa swimming in sea water. If sea-urchin spermatozoa are photographed using dark ground illumination, as in figure 9, the heads appear as brilliant patches of light on a dark background. The exposure time for figure 9(*a*) was 1/500th second, which explains why the deformations in the sperm tails are 'frozen' in position. If, however, the exposure is ½-second, the resultant picture is very different (figure 9(*b*)). In this photograph, spermatozoa are 'peeling off' from a drop of semen into sea-water, and the lengths of the white lines or tracks show how far the heads of the spermatozoa moved during the ½-second exposure. J. Gray [8] has shown that the sperm head tracks obtained in these circumstances are not spiral, although they look as if they are, but loops in a single plane. The looped tracks are caused by the peculiar movements of the sperm head (figure 2), which, at certain moments in each cycle, may actually go backward, even though the spermatozoon as a whole moves forward, relative to the ground.

Among the many questions which automatically come to mind when studying spermatozoa, one of the most important and interesting concerns the mechanism of their movements. Although the hydrodynamics of sperm movement are reasonably well understood, through the work of Gray [7, 8] and G. I. Taylor [15, 16], there is virtually no information about the changes within the tail responsible for the observed waves of bending. The first move in tackling this question is to investigate the morphology of the sperm tail. It is only in recent years that this has become possible, because the structures within the tail of a spermatozoon are too small to be seen with the light

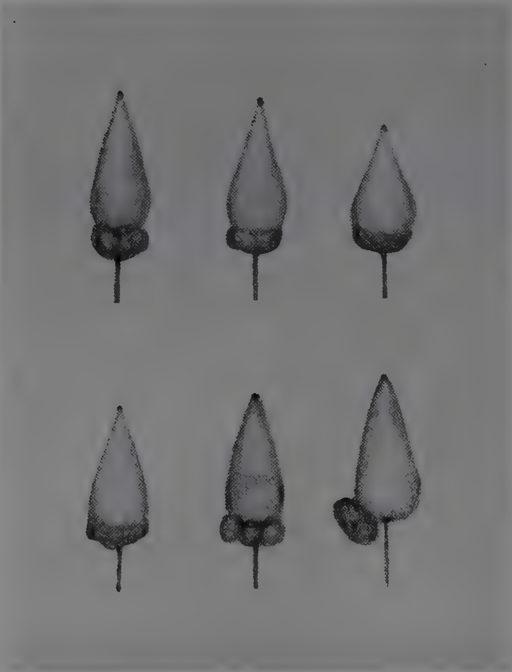


FIGURE 1 – *Drawings of spermatozoa of a sea-urchin* (Echinus esculentus), *by Retzius* [11].

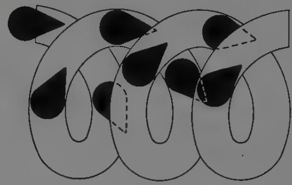


FIGURE 2 – *Diagram showing the movements of the head of a spermatozoon of a sea-urchin* (Psammechinus miliaris), *and the track formed by these movements when a swimming spermatozoon is photographed using dark ground illumination and a long exposure time, as in figure* 9(*b*). *Only some of the head positions have been included. After Gray* [8].

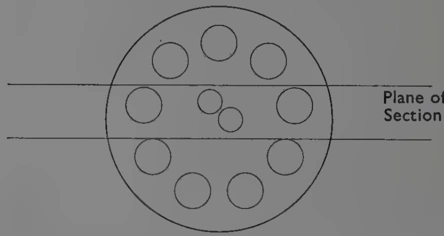



FIGURE 3 – *Diagram showing the plane of the section* (*at right angles to the plane of the paper*) *of the electron micrograph reproduced in figure* 6.

microscope. It seems to be a general rule, both in the animal and plant kingdoms, that vibratile protoplasmic structures, such as cilia and flagella, consist of nine fibrils in a circular array, embedded in an approximately cylindrical protoplasmic matrix, with two smaller fibrils running along the longitudinal axis of the cylinder. J. R. G. Bradfield [2] was the first to show that the sea-urchin sperm tail conforms to this ubiquitous pattern. Whether the nine fibrils, which are about 300 Ångströms in diameter, or the two central ones, which are about 250 Ångströms in diameter, are solid or tubular is a matter about which there is no certainty. Moreover, in an electron micrograph of a transverse section of the tail of a sea-urchin spermatozoon published by B. A. Afzelius [1], it appears that the two central fibrils are single, but that eight of the nine outer ones are double and the remaining one triple, which means that there are in reality nineteen fibrils in the outer ring, in nine groups. The chances of obtaining a reasonable length of the tail in longitudinal section are, for obvious reasons, remote. The electron micrograph reproduced in figure 6, of an approximately longitudinal section of part of a sea-urchin spermatozoon, shows that, at any rate in the neighbourhood of the head, there is no evidence of either the outer or central fibrils being twisted or helically disposed along the tail axis. But if the tail fibrils were twisted in such a way that one complete turn occupied the length of one tail wave, i.e. about 30 μ (figure 9(*a*)), there would be no chance of observing any twist in the short length of tail available for study in figure 6. This question is important when considering the mechanism of sperm movement.

There is a dark line running down the middle of what looks like one central tail fibril in figure 6. This line is almost certainly due to the plane of the section being such that the two central fibrils overlap along this line, as shown in figure 3.

The transverse section of a tail, with a white ring round it in figure 6, contains only two fibrils. This section was probably located near the extreme end of the sperm tail. According to Afzelius [1], some sections at the extreme end of the tail contain no filaments at all.

The most reasonable hypothesis to explain the waves of bending which pass along the tail of a swimming spermatozoon is that elements in the

FIGURE 4 – *A male sea-urchin* (Echinus esculentus) *spawning under water.* (*Natural size.*)

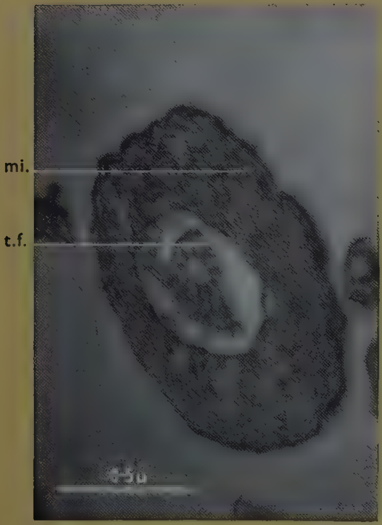



FIGURE 5 – (*a*) *Transverse section of the middle-piece of spermatozoon of a sea-urchin* (Echinus esculentus). *The section is in the neighbourhood of the centriole. Note that the two central fibrils are absent. Fixed with osmium tetroxide. t.f., outer ring of tail fibrils; mi., mitochondria.*

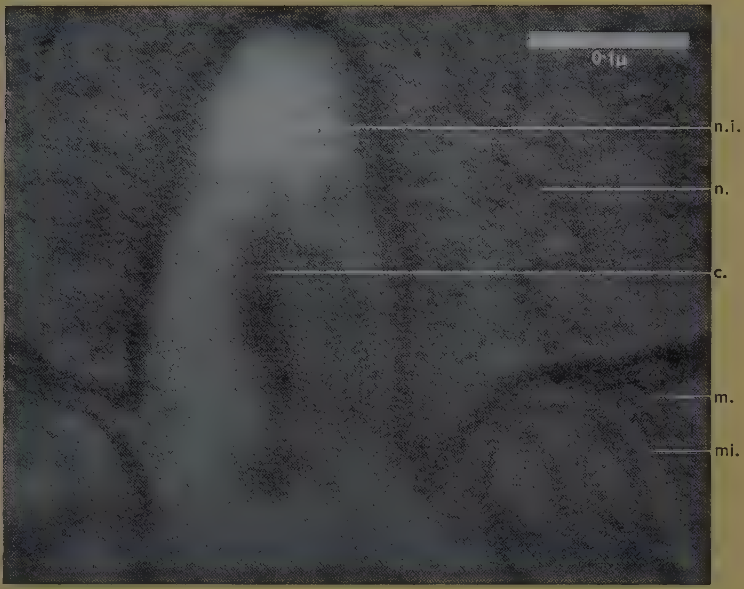



FIGURE 5 – (*b*) *Longitudinal section of the posterior end of the head of spermatozoon of a sea-urchin* (Echinus esculentus). *The electron micrograph shows the nuclear invagination (n.i.) which normally contains the centriole (c), part of which is visible. Note mitochondria (mi.) in the middle-piece (m.) bottom right. n., nucleus. Fixed with osmium tetroxide.*

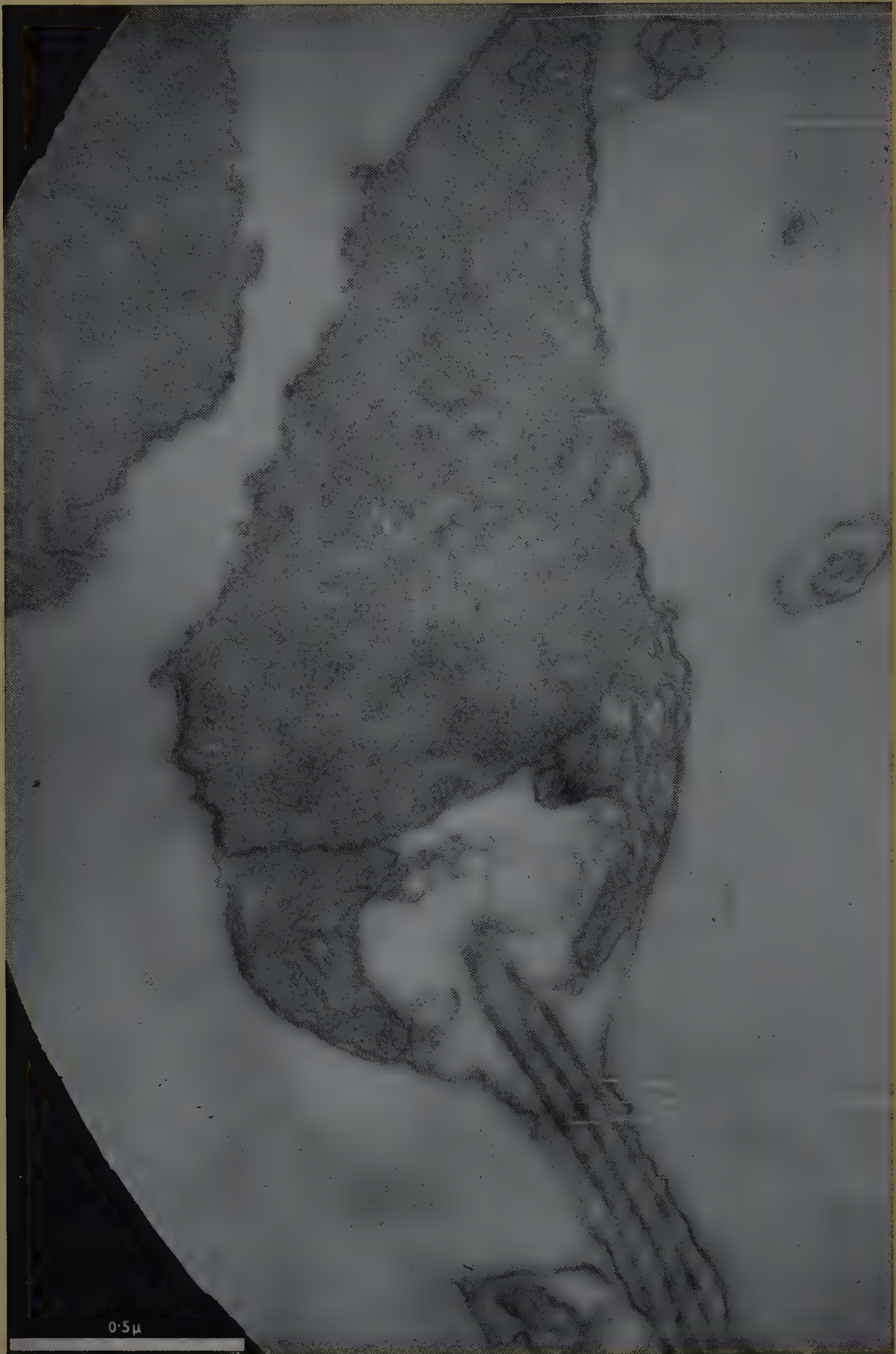



FIGURE 6 – *Longitudinal section of the head, middle-piece, and part of the tail of spermatozoon of a sea-urchin* (Echinus esculentus). *The structure within the white circle is a transverse section near the extreme posterior end of the tail. Fixed with osmium tetroxide. c.m., cell membrane; c.n.m., cell and nuclear membranes; h., head; m., middle-piece; n.m., nuclear membrane; t., tail; a.n.i., anterior nuclear invagination; t.f., tail fibrils; t.s., transverse sections of tail. The upper and lower ones were cut at the posterior end of the middle-piece.*

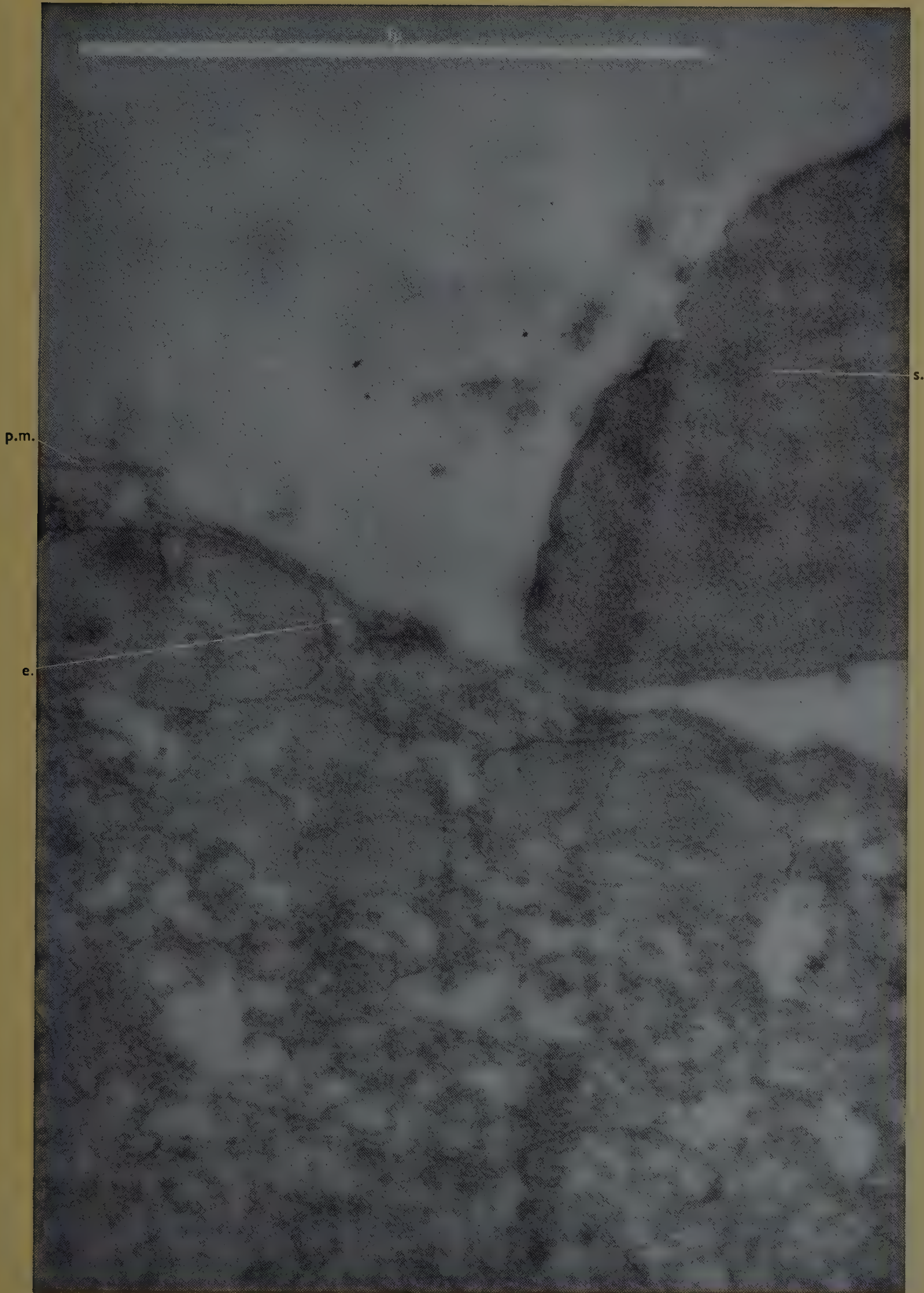
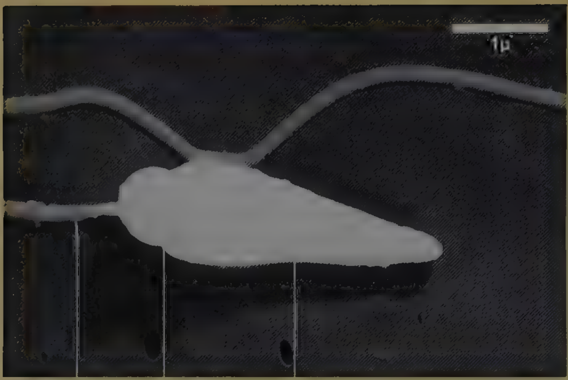



FIGURE 7 – *Spermatozoon of* Echinus esculentus *on or very near the surface of an egg of the same species. Fixed with osmium tetroxide* 25 *seconds after mixing unfertilized eggs and spermatozoa. Note apparent disappearance of plasma membrane (?) in the neighbourhood of the anterior end of the spermatozoon. e., egg; s., spermatozoon (head); p.m., plasma membrane (?).*



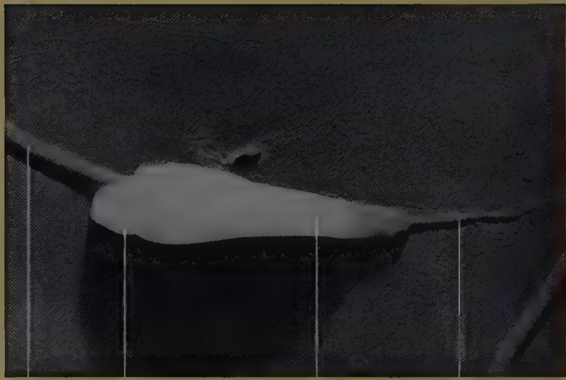




FIGURE 8 – (*left*) *Spermatozoon of a Japanese sea-urchin* (Hemicentrotus pulcherrimus) *in sea-water. The semen was diluted with sea-water and the suspension fixed with formalin* $1\frac{1}{2}$ *minutes later* (*J. C. Dan* [4]). *h., head; m., middle-piece; t., tail.* (*right*) *Spermatozoon of* Hemicentrotus pulcherrimus. *The semen was mixed with sea-water and egg secretions were added one minute later. This caused agglutination of the spermatozoa and the 'extrusion' of the acrosomal filament. Reversal of agglutination occurred* $3\frac{1}{2}$ *minutes later, and after* 4 *minutes the suspension was fixed with formalin* (*Dan* [5]). *a., acrosomal filament; h., head; m., middle-piece; t., tail.*



FIGURE 9 – (*a*) (*left*) *Spermatozoa of a sea-urchin* (Psammechinus miliaris) *swimming in sea-water. Photomicrograph by J. Gray and M. M. Swann. Exposure time, 1/500th sec.; dark ground illumination.* (*b*) (*above*) *Semen of sea-urchin* (Psammechinus miliaris) *in sea-water at 17°. At the edge of the white drop of semen, spermatozoa are 'peeling off' into the water. The apparently spiral tracks are caused by the translatory movements of the sperm heads during a photomicrographic exposure of 0·5 seconds. Dark ground illumination.* (*Rothschild and Swann* [12]).

nine outer fibrils shorten periodically in a particular order [8]. If elements in the tail execute active bending (which involves their contraction), there must also be other structures in the tail which resist compression. The compression-resisting structures might be the protoplasmic matrix in which the fibrils are embedded, or the tail membrane, or the two central fibrils. From Gray's analysis of the movements of sea-urchin spermatozoa it appears probable that the nine outer fibrils are capable of active contraction. But if they run straight along the whole length of the sperm tail, complicated machinery analogous to a nervous control mechanism must exist to ensure that the fibrils contract in the right order and at the right time at different points along their long axes. Less complicated machinery is required if the fibrils are twisted, because, in these circumstances, each fibril need only contract as a whole, in a particular order, to achieve waves of bending. Bradfield [3] has raised the possibility that the two central fibrils, which are chemically different from the nine outer ones [9], may be responsible for the conduction of 'nerve impulses' along a cilium. As mentioned earlier, the amount of twist required to obviate the need for this sort of machinery is so slight as to be difficult to observe. In the absence of more information, the simpler hypothesis, involving twisted fibrils, should therefore not be ignored, at any rate so far as the tails of spermatozoa are concerned.

The tail of the sea-urchin spermatozoon passes through the mid-piece and terminates in an invagination of the posterior part of the nucleus that can be seen in figure 5(*b*) but not in figure 6, in which the plane of the section is not sufficiently central to reveal the cavity. The tail became detached during preparation of the section shown in figure 5(*b*). The anterior end of the tail terminates in the centriole, which, according to Afzelius [1], is a 'curved disk', with a diameter of 0·15 μ. Afzelius says that the nine outer fibrils can be identified at the edge or margin of the centriole, while the two central fibrils arise in the middle of it. In several transverse sections cut in the region of the centriole I have, however, noticed that the two central fibrils are missing (figure 5(*a*)). Although this may have been due to some accident during preparation of the sections, the same thing has been observed at the anterior ends of cilia [6], and it therefore seems probable that Afzelius's view that the central fibrils extend into the centriole is based on misinterpretation of his electron micrographs.

The centriole is, presumably, the structure from which orders are sent out to the fibrils to contract. We have no idea whether these orders are chemical or electrical, nor how they are sent out, and it will be some time before any experimental techniques will be available to investigate this question. Bradfield [3] has published a speculation about this subject, but the complexity of his model is in part due to his belief, which is shared by Afzelius, that there is no possibility of the nine outer fibrils being helically disposed along the sperm tail (or the long axis of a cilium).

### MID-PIECE

This structure is seen in longitudinal section in figure 6 and in transverse section in figure 5(*a*). It is bounded by a double membrane, 100 Ångströms thick, and contains mitochondria of a very characteristic type. The mid-piece of a spermatozoon is believed to contain the enzyme systems and internal fuel supplies necessary for sperm movement, though there is little experimental evidence in support of this view. K. Cleland and I [13] tentatively came to the conclusion that the mid-pieces of sea-urchin spermatozoa which had exhausted their internal fuel supplies were smaller than those of fresh spermatozoa. We also found that the mid-piece contained quantities of lipid, a suggestive finding as sea-urchin spermatozoa metabolize phospholipid to provide energy for their movements. Apart from these fragmentary observations, little is known about the functions of the mid-piece, apart from inferences based on its mitochondria-like contents.

### HEAD

The function of a spermatozoon is to stimulate the egg to develop and to provide the future embryo with genes of paternal origin. The latter function is discharged by the sperm nucleus, which constitutes the major part of the head. The nucleus is bounded by a double membrane, about 150 Ångströms thick, outside which there is another, somewhat thinner, membrane which covers the whole surface of the spermatozoon (figure 6).

When a spermatozoon fertilizes an egg, it first becomes attached to the egg surface and later penetrates into the inside of the egg. To do this it must pass through the plasma membrane, which is probably composed of lipids or lipoproteins. This membrane can be seen in figure 7, which shows a spermatozoon attached, or just about to become attached, to the surface of an egg. The

dissolution of the plasma membrane at the point of attachment of the fertilizing spermatozoon is probably achieved by a substance which can be extracted from sea-urchin spermatozoa and is called Sperm Lysin (I), or Androgamone III (A. III). A. III haemolyses red blood cells and has an effect on the surface of sea-urchin eggs which is similar to that of bee venom and detergents [14]. It may be a long-chain unsaturated fatty acid. There was, until recently, little direct information about the location of A. III in sea-urchin spermatozoa, though for obvious reasons the most probable location is the front of the head. Figure 6 shows that the tip of the sperm head is morphologically differentiated from the rest of the head, which contains nuclear material. In electron micrographs published by Afzelius [1] there is a spherical structure, which is barely visible in figure 6, above the nuclear invagination. It is tempting to believe that A. III is located in this part of the sperm head, either in the spherical body or the invagination. The dissolution of the egg plasma membrane can be seen in figure 7, in the neighbourhood of the anterior end of the sperm head. To the right and left of this region the membrane is intact, while some substance appears to connect the sperm head with the broken down region of the egg surface. In the original electron micrograph this 'substance' is a spherical body, somewhat smaller in diameter than that reproduced by Afzelius. Although the section of the spermatozoon in figure 7 is somewhat oblique, so that the extreme tip of the head is above or below the plane of the section, one has to be rather lucky to arrest fertilization at the moment of attachment of the spermatozoon to the egg surface, find the egg in question, and then cut a section in the right plane. This is unfortunate, because it means that the examination of a number of eggs in precisely this condition, which would be desirable before making any generalizations, presents considerable difficulties.

In some species of sea-urchins, a filamentous process, sometimes called the acrosomal filament, is ejected from the tip of the sperm head when the spermatozoon comes into contact with water in which eggs have been standing. Figure 8 (left) shows what the spermatozoon of a Japanese sea-urchin, *Hemicentrotus pulcherrimus*, looks like when in normal sea-water. Figure 8 (right) is an electron micrograph of a spermatozoon of the same species which has been suspended in egg water, i.e. sea water in which eggs have been standing. Secretions from the unfertilized eggs into the sea water have catalysed the ejection of the acrosomal filament. The relationship between this filament and the structure at the tip of the sperm head in figure 6 is not clear. Does contact with egg water make the invaginated structure behave like the finger of a rubber glove which, having been pulled inside out when taking the glove off, is blown out the right way? More experiments are needed to resolve this question, but so far as the spermatozoa of *Echinus esculentus* are concerned, the existing evidence does not suggest that the invaginated structure is blown inside out when the spermatozoon comes in contact with the egg surface.

## CONCLUSION

'Modern' biologists sometimes adopt a lofty attitude towards morphologists, making unfavourable comparisons between their work and that of biochemists and biophysicists. I hope that this article will help to dispose of the idea that morphology is a dull subject. It is as exciting today as in the seventeenth century, when Leeuwenhoek was studying the morphology of his spermatic animalcules.


## ACKNOWLEDGMENTS

I am indebted to Professor N. F. Mott, F.R.S., for permission to use the Siemens electron microscope in the Cavendish Laboratory, University of Cambridge; to Mr R. W. Horne for his invaluable assistance in taking the electron micrographs; to Professor Sir James Gray, F.R.S., for many stimulating discussions; and to the Medical Research Council for providing me with a research assistant.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# The velocity of light

L. ESSEN

Probably no physical constant is of more fundamental importance in physical theory and practice than is the velocity of light, and this is reflected in the great effort which has been expended in its measurement. Apparent discrepancies between some of the earlier determinations suggested that the velocity of light is not strictly constant, but depends upon the time of the determination and the frequency of the radiation used. Analysis of the results of the most modern methods, however, gives no support to this suggestion.

The velocity of light was first measured, in 1676, by the astronomer Roemer, who correctly attributed the irregularities in the times of the eclipses of Jupiter's satellites to the time taken for the light to travel along the diameter of the Earth's orbit round the Sun. Another astronomical method, depending on the apparent shift of star positions with the Earth's motion, was used by Bradley, fifty years later, but it was not until 1849 that Fizeau obtained a value from observations of the time of travel of a pulse of light on the Earth's surface. In Fizeau's famous experiment the light was chopped by a toothed wheel rotating in front of the source, and reflected by a mirror at a distance of about 9 km. If the returning pulses of light passed through a gap between the teeth of the wheel a bright image was seen in a telescope behind it, but if they fell on a tooth the intensity of the image was a minimum. The speed of the wheel was adjusted to give this condition of minimum intensity, and the velocity was then calculated from the distance travelled, the rotational speed of the wheel, and the number of teeth.

During the latter half of the nineteenth century the value of the constant was required for the electrical calculations involved in problems such as the design of submarine cables, and many measurements were made by both electrical and optical methods. This connection between the velocity of light and electrical theory is so important that it is worth while recalling how it was established. The force between electric charges was found to depend on the permittivity $\varepsilon$ of the medium and that between electric currents on the permeability $\mu$, but so long as electrostatics and electromagnetics were treated as separate subjects it was not necessary to know the relationship between the two constants; by a suitable choice of unit, the values in a vacuum, $\varepsilon_0$ and $\mu_0$, could both be made equal to unity. In electromagnetic problems, however, it is essential to know the ratio between quantities expressed in the two systems of units, and this ratio, in the case of electric charge for example, turns out to be $(\mu_0\varepsilon_0)^{-\frac{1}{2}}$, which has the dimensions of a velocity. The ratio was determined by measuring a charge in the two units and was found to be, within the experimental errors, equal to Fizeau's value for the velocity of light. Faraday, with his brilliant insight, had already suggested that light might be a manifestation of electromagnetic energy, and Maxwell formalized the idea in his famous electromagnetic theory of light, according to which the velocity of all electromagnetic waves *in vacuo* is $(\mu_0\varepsilon_0)^{-\frac{1}{2}}$ and is thus independent of the frequency of the waves. This conclusion was supported by all the results obtained. The most accurate electrical measurement, made by Rosa and Dorsey, gave a value of 299 784 $\pm$ 30 km/s. The most famous of the classical optical experiments, carried out by Michelson, Pease, and Pearson, using a light beam in an evacuated metal pipe a mile long, gave 299 774 $\pm$ 11 km/s.

In 1941 R. T. Birge [1] reviewed the experimental work, and concluded that the most probable value was 299 776 $\pm$ 4 km/s, and this value was generally believed to be sufficiently accurate for the new applications in nuclear physics, radar, and radio engineering which were then becoming of great importance. But to someone who, like the writer, had been engaged in precise electrical measurements, limits of $\pm$ 4 km/s seemed too optimistic in view of the large observational scatter of about $\pm$ 50 km/s. It was therefore decided to make a new determination, using an instrument known as a cavity resonator, which was developed during the last war for measuring the frequency of very short waves. It consists of a hollow metal cylinder, or waveguide, closed at both ends by metal plates, and electrically coupled to an oscillator and a detector through small holes in the walls. Electrical resonance occurs when the length

of the cavity is $n\lambda_g/2$, $\lambda_g$ being the wavelength of the radiation in the guide and $n$ being an integer. This condition, which can be recognized by a sharp increase of the signal in the detector, can be established by varying either the frequency of the oscillator or the length of the cavity. The value of the instrument rests on the extreme sharpness of the resonance, which makes it possible to set the frequency or length with a precision of 1 part in $10^6$ if suitable precautions are taken. It was this precision of setting which made the cavity resonator method appear particularly attractive in comparison to the optical method.

There is a simple relationship between the resonant frequency, the dimensions of the guide, and the quantity $(\mu_0\varepsilon_0)^{-\frac{1}{2}}$; the frequency can therefore be calculated if a value be assumed for $(\mu_0\varepsilon_0)^{-\frac{1}{2}}$ which can be taken as the velocity of light *in vacuo*. It is more convenient to calibrate the instrument by measuring, directly in terms of a frequency standard, the frequency of the oscillator corresponding to a particular setting; if the dimensions and the frequency are both measured, a value for the velocity is obtained.

The first determination by this method was made in 1946 at the National Physical Laboratory by L. Essen and A. C. Gordon-Smith [2], using the apparatus shown in figure 1. The cavity resonator was of the simplest possible geometrical construction, and was mounted in an evacuated enclosure so that the result need not be corrected for the refractive index of air. The frequency of the oscillator was varied slowly in the region of

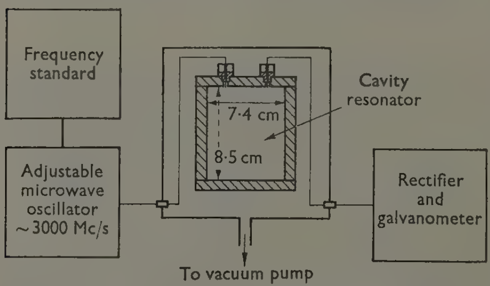


FIGURE 1 – *Determination of the velocity of light by means of a cavity resonator of fixed dimensions.*

3000 Mc/s (wavelength 10 cm) until resonance occurred, and a reading was obtained on the galvanometer scale. The frequency of the oscillator corresponding to the peak of the resonance was measured by reference to the standard, and the dimensions of the cavity were determined by careful metrological measurements. The repetition accuracy of this preliminary experiment was ± 1 km/s—about fifty times better than that of any previous determination—but there was evidence of small systematic errors. These were reduced [3] by using a form of resonator shown in figure 2. The length of the resonator was varied by moving a piston, and successive resonances were obtained at intervals of $\lambda_g/2$. The distance moved was measured by means of gauges placed between the end of the piston rod and a fixed base

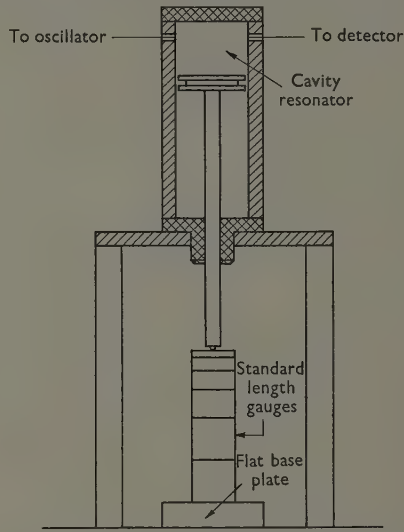


FIGURE 2 – *Cavity resonator of variable length.*

plate. The length for the first resonance is not exactly $\lambda_g/2$ because of end effects, but the true value is obtained from the difference between successive settings. Moreover, by making measurements at different frequencies—the values actually used were between 5000 Mc/s (6 cm) and 11 000 Mc/s (2·7 cm)—it was possible to eliminate both the diameter and the wall resistance from the calculations and to express the result entirely in terms of frequencies and lengths. This was a very important feature of the experiment, because it is very difficult to calculate the effects of the inevitable mechanical and electrical imperfections of the surfaces.

A further cavity resonator result obtained by K. Bol [4] at Stanford University is 3 km/s lower than that obtained at the National Physical Laboratory; this small discrepancy may be due to the fact that Bol was not able, with his form of cavity, to eliminate experimentally the effects of wall imperfections.

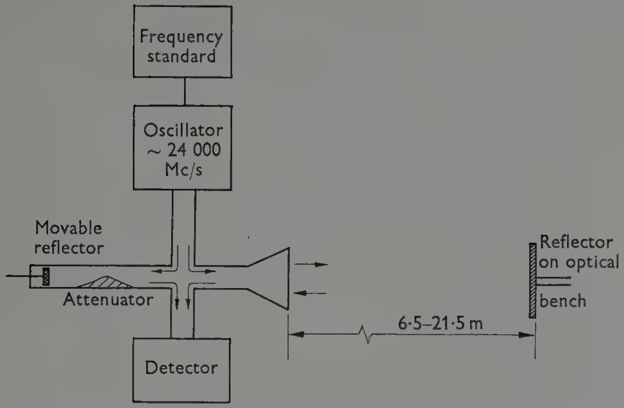


FIGURE 3 – *Determination of velocity of light by a Michelson type interferometer for radio waves.*

In more recent measurements at the National Physical Laboratory K. D. Froome [5] has made use of a microwave form of Michelson interferometer (figure 3). Radio waves approximately 1·25 cm long (24 000 Mc/s) travel down a waveguide to a junction, where they are split into two equal beams. One beam is radiated into the open air from a horn and a portion of it is returned from a distant reflector, through the horn to the junction, and thence to the detector. The other beam is contained within a waveguide network, and is reflected back to the junction and to the detector after being attenuated in its path by an amount equivalent to the attenuation of the beam in the open air. The two beams of equal strength are thus recombined at the detector, and the intensity of the signal depends on their relative phases. If the open air reflector is moved towards the horn the phase of the beam will cycle through 360° for each half-wavelength moved, and the detected signal will pass through maximum and minimum values. The distance moved can thus be expressed in terms of wavelengths of the radiation, and as the frequency also can be measured with great precision the velocity can be found ($c = \lambda f$). There are two important corrections to be applied, one due to the refractive index of air, and the other to the diffraction of the wave. The refractive index was measured by Essen and Froome [6] in a preliminary experiment with an accuracy of 1 part in $10^7$, and the diffraction error, on the average about 16 km/s, was determined by a combination of theory and experiment. Froome expects [7] that a further measurement of this kind using shorter waves will give a value with a maximum error of at most $\pm$ 1 part in $10^6$ ($\pm$ 0·3 km/s).

There has always been speculation, on rather inadequate evidence, about the variation of the velocity of light with time or with frequency. It is fortunate, therefore, that the recent results, nearly all of which differ by about 17 km/s from Birge's value, have been obtained by a number of completely different techniques using a wide range of frequencies. In addition to those already mentioned E. Bergstrand [8] has carried out several vastly improved optical measurements with an instrument, called a geodimeter, designed for surveying purposes. A parallel beam of light waves, modulated at a frequency of 10 Mc/s by a Kerr cell, is reflected back from a mirror about 10 km distant. The reflected wave is detected by a photomultiplier tube, the sensitivity of which is varied at the modulation frequency. The strength of the detected signal therefore depends on the phase of the returning wave and hence on the distance travelled. Two further features make the method a practical success. The beam is modulated by a 50 c/s square wave in such a way that for alternate half-cycles of 0·01 second duration the phase of the 10 Mc/s modulation is reversed. There are thus in effect, travelling in the same path, two beams, which are combined in opposition by the detector to give a sharp zero, instead of a flat minimum, when the distance travelled is a whole number of half-wavelengths of the modulation. The other feature is a short calibrated artificial path formed by multiple reflections between a set of prisms, and this enables the zero of the instrument to be found. It seems likely that the geodimeter will in some applications replace the familiar tape methods of establishing base lines, the distances being determined from the times of travel and velocity of pulses of light, the value for velocity being assumed. But in these initial experiments it has been used on well established geodetic base lines, the lengths of which are known, and has given accurate values for the velocity of the light waves. A geodimeter was lent by the United States to the British Ordnance Survey in 1953 for experimental checks on the Ridgeway and Caithness base lines, and the results obtained were in close agreement with the geodetic values [9]. As the instrument was previously calibrated at the National Physical Laboratory the measurements can be regarded as an independent measurement of velocity giving a result 0·8 km/s less than that obtained by Bergstrand.

An important series of determinations made by C. J. Aslakson [10] also employed a technique developed for surveying purposes, but operated over much greater distances. This is an adaptation of the Shoran radar system using frequencies near 300 Mc/s (wavelength 1 m). In normal use a pulse of radio waves transmitted from an aeroplane is returned from two ground stations, and the times taken are read from a time scale on the screen of a cathode-ray tube formed by time markers from a quartz oscillator. The distances of the plane from the two ground stations are computed from the times of travel and the velocity. In wartime practice the time scales were adjusted to read distance directly, on the assumption of the optical value of velocity (299 776 km/s, corrected for the refractive index of air), and the position of the plane was found from the two distances. In Aslakson's experiments the ground stations were situated at points whose distance apart, of the order of 100 miles, had been determined accurately by geodetic methods. The plane flew across the line joining them, and the minimum distances were determined. If the height of the plane is also known, the ground distance between the stations can be calculated, a comparison between the measured values then giving the error in the value of velocity assumed in the calibration of the distance scale. One of the main difficulties was the correction for the refractive index of air, which varies considerably over the path. However, Aslakson was finally able to obtain consistent results, and radar methods have now been used both in the United States [11] and in Canada [12] for large-scale surveying.

*Results of recent determinations of the velocity of light* ($c_0$)

| *Date* | *Author* | *Method* | *Result (km/s)* | *Experimental scatter (km/s)* |
|---|---|---|---|---|
| 1941 | Birge | Discussion of results up to 1941 | 299 776 ± 4 | About 100 |
| 1948 | Essen and Gordon-Smith | Cavity resonator | 299 792 ± 3 | 7 (including systematic effects) |
| 1949 | Bergstrand | Optical, Kerr cell geodimeter | 299 796 ± 2 | 4·8 (between nightly means) |
| 1949 | Aslakson | Radar | 299 792·4 ± 2·4 | 11 |
| 1950 | Houstoun [14] | Optical, quartz plate | 299 775 ± 9 | 180 |
| 1950 | McKinley | Optical, quartz plate | 299 780 ± 70 | [1] |
| 1950 | Bergstrand | Optical, Kerr cell geodimeter | 299 793·1 ± 0·25 | 5 |
| 1950 | Essen | Cavity resonator | 299 792·5 ± 1 | 1·5 (including systematic effects) |
| 1950 | Bol | Cavity resonator | 299 789·3 ± 0·4 | [2] |
| 1951 | Bergstrand | Optical, Kerr cell geodimeter | 299 793·1 ± 0·2 | 5 |
| 1951 | Aslakson | Radar | 299 794·2 ± 1·9 | 13·5 |
| 1951 | Froome | Radio interferometer | 299 792·6 ± 0·7 | 2·5 |
| 1952 | Rank, Ruth and Vander Sluis | Spectral lines | 299 776 ± 7 | [3] |
| 1954 | Froome | Radio interferometer | 299 793·0 ± 0·3 | 0·7 |
| 1954 | Rank, Shearer and Wiggins | Spectral lines | 299 789·8 ± 3 | [3] |
| 1954 | Florman [16] | Radio interferometer | 299 795 ± 3·1 | [3] |
| 1954 | Plyler [15] | Spectral lines | 299 792 ± 6 | [3] |
| | | Most probable value | 299 793·0 ± 0·5 | |

[1] Work carried out in 1937–38.
[2] Details not given.
[3] Result obtained by a least squares analysis of the observations.

There is one further method, used by D. H. Rank and his collaborators [13], which should be mentioned because of its intrinsic interest and potential importance. It is based on the determination of the molecular constants of hydrogen cyanide by both optical and microwave spectroscopy. The results in one case are expressed in wavelength units, and in the other in terms of frequency; by combining the two the velocity, $c_0$, can be obtained.

These recent measurements by such very different methods have shown that there is no detectable change in the value of $c_0$ with frequency, and if realistic limits are placed on the earlier measurements there is also no evidence of any change with time.

It has often been asked how such a large error in the value of this important constant could remain undetected for so long, and it is important to find an answer to this question. The first point to remember is that prior to 1946 the values were obtained as the averages of large numbers of observations having a spread of about $\pm$ 50 km. In these circumstances it was not possible to investigate small systematic errors, although in two of the most important measurements, those by Michelson, Pease, and Pearson and by Anderson, the authors themselves called attention to such errors. In spite of these warnings, and perhaps misled by the elaborate scale of the experiments and the close agreement between the values obtained with very different apparatus, most reviewers of the subject gave their estimate of the most probable value with very small limits of error. But the evaluation of errors is very difficult even for the experimenters themselves, and must be far more difficult for anyone not having practical experience of the method. It is therefore not really surprising that the reviewers should reach erroneous conclusions about both the value and its accuracy. Moreover, the work of reviewers saves other scientists much tedious labour, and when their findings have been published the original papers are seldom referred to again. Any mistakes tend therefore to be perpetuated. Their value was in this instance so firmly held that a common reaction to the first cavity resonator result, in spite of its high accuracy compared with any previous determination, was that it was an interesting new experiment which would no doubt give the correct answer when the technique was perfected.

In the list of values given in the accompanying table the scatter of the experimental results is indicated, where possible, in addition to the limits given by the authors, in order to convey an idea of the precision of measurement.

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# The Perkin family of organic chemists

SIR ROBERT ROBINSON

This year is the centenary of the discovery of mauve, the first synthetic dye, by Sir William Perkin. The discovery had little significance in itself, but its possibilities—which Perkin had both the vision to see and the perseverance to translate into reality—were immense. He founded not only the manufacture of synthetic dyes but the whole organic chemical industry. Chemical talent was happily a family attribute and with two of his sons—William Henry junior and Arthur George—he played a major part in the development of organic chemistry in Britain. The achievements of this outstanding family are here saluted by a brilliant successor.

It is probable that the first laboratory designed for the teaching of chemistry was established by Lomonosov (1748) at St Petersburg, but the model for all later institutions was set up by Liebig at Giessen in 1824 and almost the whole later development of organic chemistry stemmed from this source. It seems, however, that one can have too much of a good thing, and when Liebig was invited to Munich in 1852 he accepted the call subject to the stipulation that he would be relieved from teaching duties.

A. W. von Hofmann, who entered the University of Giessen in 1836 as a law student, was soon attracted into Liebig's orbit and devoted himself to chemistry with outstandingly successful results; in 1843 he became Liebig's personal assistant. At about this time the authorities in England were seeking a professor for the Royal College of Chemistry, later absorbed into the Imperial College of Science. In view of the reputation of Liebig's school of agricultural chemistry it is not surprising that they sought to fill the post with a worthy representative of this group. The nomination of Hofmann for the professorship, in which the Prince Consort took an active interest, was made by Liebig. The period of nineteen years spent by Hofmann in England was of fundamental importance for three reasons.

First, the teaching of organic chemistry and the inspiration of organic chemical research in Britain derived very largely from Hofmann and his associates and pupils, although it is, of course, true that, as in a cyclotron, additional impulses of very great importance were received from time to time.

Second, the actual discoveries made—to quote but one example, in the chemistry of the amines—consolidated the theory of the infant science of the structural chemistry of carbon compounds.

Third, the foundations of the organic chemical industry were well and truly laid.

In all these developments, William Henry Perkin and his sons, William Henry and Arthur George, made essential contributions of different kinds and in varying degree. Sir William Perkin was not only a great organic chemist and discoverer, he was also a courageous man of character and determination. He must be reckoned among the great pioneers and he took frightening risks in trusting the clarity of his vision and the soundness of his judgment, often against the advice of his seniors, including Hofmann himself. William Henry Perkin, jun., can justly be estimated as a greater organic chemist than his father, but he sailed in relatively quiet waters and devoted himself almost exclusively to pure scientific work. Arthur George Perkin was also a star of the first magnitude, although brilliance is less apparent in this small constellation than it would be in comparison with other contemporaries.

The forebears of the Perkins were yeoman farmers in Yorkshire or Cumberland, and it is of interest that Thomas Perkin (born 1757) was locally reputed to have been interested in chemistry or alchemy: on the occasion of a pilgrimage to his grandfather's house near Ingleton, Sir William found a cellar fitted as a laboratory. Thomas moved to London, where he engaged in boat building and other forms of carpentry. His son, George Fowler Perkin, (born 1802), continued and extended this business, and William Henry Perkin was born at King David's Fort, Shadwell, on 12th March 1838. It was naturally hoped that this seventh son would carry on his father's trade, but, in his own words (New York, 1906)—'When I was between twelve and thirteen years of age, a young friend showed me some chemical experiments and the wonderful power of substances to crystallize in definite forms, and the latter, especially, struck me very much, with the result that I saw there was in chemistry something

far beyond the other pursuits with which I had previously been occupied. The possibility also of making new discoveries impressed me very much. My choice was fixed, and I determined, if possible, to become a chemist and I immediately commenced to accumulate bottles of chemicals and to make experiments'.

His liberal-minded father was difficult to persuade that this decision was a wise one, but eventually withdrew all opposition and William left school at the age of fifteen, entered the Royal College of Chemistry in Oxford Street, London, and studied under Hofmann. He completed his first original investigation during his seventeenth year, and published his first paper in the Journal of the Chemical Society in 1856. His enthusiasm was such that he worked at home in the evenings and on holidays, in a laboratory which, though not lavishly equipped, was evidently quite adequate for many purposes. Here, at King David's Fort, he first investigated the reduction products of dinitrobenzene and dinitronaphthalene. The very interesting outcome of this work, done jointly with Church, was the discovery of a red substance 'nitrosonaphtyline' to which Hofmann himself adverted in 1856 and which Perkin and Church showed in 1857 could be produced more simply from a naphthylamine salt by the action of a nitrite. We now know that this substance is aminoazonaphthalene and it was the first definite azo-compound to be isolated. The dyeing properties of the substance were recognized and a patent was taken out in 1863. This is also of historic interest, in that it claims for the first time the production of a sulphonated azo-dye, made by applying the nitrite reaction to naphthylaminesulphonic acid. It remained for Peter Griess, also a student of Hofmann, to discover the aryldiazonium salts and to elucidate the mechanism of formation of the red substance of Perkin and Church.

It is stated in some texts that the discovery of 'mauve' was made by Perkin when working in Hofmann's laboratory, and the master's supervision is implied. This is not true: the work was independently contrived and executed in the home laboratory. However, it was the result of a remark of Hofmann that the synthesis of quinine ought to be possible and would be very desirable. At this time the relations of the molecular weights of simple organic compounds, such as alcohol and ether, had not long been established, chiefly by the admirable work of Williamson in 1852, and in regard to atomic weights this was the period of the classical paper of Cannizzaro. Very little was known about the molecular structure of any cyclic compound and, in fact, the whole splendid edifice of structural organic chemistry as we know it today, has been erected during the past century. In 1856, it was thought that a certain empirical composition was characteristic of each pure compound and hence that in order to synthesize a substance all that was necessary was to make a molecule having the desired composition. Thus, it was argued, quinine, $C_{20}H_{24}O_2N_2$, might be produced by the oxidation of two molecules of $C_{10}H_{13}N$ as shown in the equation:

$$2C_{10}H_{13}N + 3O = C_{20}H_{24}O_2N_2 + H_2O.$$

The nearest known substance was toluidine, $C_7H_9N$; to get $C_{10}H_{13}N$ from this, a hydrogen atom must be replaced by $C_3H_5$, that is by the known allyl group. Hence Perkin treated toluidine with allyl iodide, a reaction which afforded the desired allytoluidine, $C_{10}H_{13}N$. When this was oxidized with bichromate in acid solution, a brown precipitate was thrown down. Although no quinine was obtained, Perkin was interested in the reaction and tried the same process with simpler bases; with aniline sulphate and potassium bichromate he obtained a black precipitate, which he had the curiosity to examine. Boiling alcohol extracted a mauve colouring matter from it, and he soon found that this was able to dye silk and that the dyeings were relatively fast to light and washing. All this took place between Easter and June, 1856. He sent dyed patterns to Messrs. Pullar of Perth and as early as 12th June received the following:

'If your discovery does not make the goods too expensive, it is decidedly one of the most valuable that has come out for a very long time. This colour is one which has been very much wanted in all classes of goods and could not be obtained fast in silks, and only at great expense in cotton yarns. I enclose you a pattern of the best lilac we have in cotton—it is dyed by only one house in the United Kingdom, and does not stand the tests that yours does, and fades by exposure to air'.

Thus encouraged, Perkin never looked back. With the most admirable perseverance, assisted by his elder brother, Thomas D. Perkin, he overcame one formidable obstacle after another, and founded not only the manufacture of synthetic dyes but also the whole organic chemical industry. He quickly decided to attempt the commercial exploitation of his discovery and was greatly helped by his father who, in Perkin's words, 'very nobly risked most of the capital he had accumulated in a

life of great industry in order to build and start works for the production'.

But Hofmann, whom he informed of his intentions in October, was discouraging, not to say annoyed, and indicated that this false step might ruin a promising career. Still Perkin persisted, not at all dismayed by his lack of experience of chemical engineering. The reason for this, mentioned by Sir William in all seriousness, was that the operations to be carried out were of such a novel character that there was little scope for imitation of existing practice. It is this active, forceful pioneering which gives Sir William Perkin his real claim to fame. The discovery of mauve was an example of sharp observation, but it had very little significance in itself. The really important thing was that the young Perkin had a vision of the possible future. It almost seems now that he was inspired with a sense of mission, and he never faltered when this took him into the unfamiliar realms of chemical technology and commerce. The generous impulse of his young blood, his quick enthusiasm, carried him away from the pursuit of pure chemistry, though only to a certain extent. He was always busy with some researches of the kind miscalled academic, and later in life returned to the fold from which he had strayed. Nevertheless, it cannot be too strongly emphasized that William Perkin was the real pioneer of the organic chemical industry not because he discovered mauve but because he set to work to manufacture and to sell it.

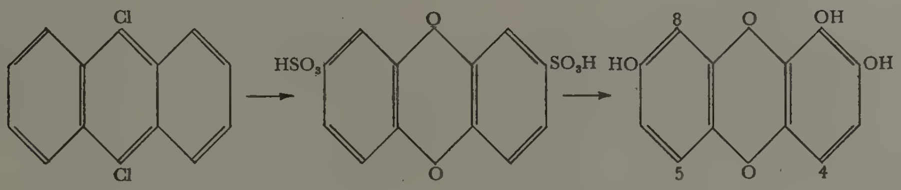
The difficulties in his way can hardly be overestimated. In the first place benzene, which had been separated from coal tar by Hofmann's pupil, Mansfield, only a short time previously, was made on only a very limited scale. A crude product was eventually secured from a Glasgow firm; it cost 5s. per gallon and had to be carefully fractionated. Again, the preparation of nitrobenzene had been accomplished only by the use of fuming nitric acid on a small laboratory scale. It was Perkin who introduced the use of a mixture of nitric acid, (*d* 1·3 or thereabouts) or sodium nitrate, and sulphuric acid, and he soon adopted cast iron vessels instead of the glass flasks hitherto thought necessary for such operations. None of these major technical advances was achieved without teething troubles, such as explosions, but Perkin's courage carried him through and his methods were soon adopted by others. For example, the firm of Simpson, Maule and Nicholson manufactured nitrobenzene (and later aniline), and Perkin and Sons obtained supplies from them when the demand for mauve became so great that the works at Greenford Green, near Harrow, could not cope with it.

In 1854 Béchamp had found that nitrobenzene could conveniently be reduced to aniline by means of iron filings and dilute acetic acid. With similar perseverance Perkin made this process a technical success and it should not be necessary to emphasize that the manufacture of aniline and its homologues greatly strengthens his claim to be regarded as the founder of the industry and not merely the fortunate discoverer of a colouring matter.

It is characteristic of the synthetic colour industry that it is subject to continuous change through the introduction of new products: this was true at the start and remains true today. Perkin's discovery of mauve stimulated others to experiments on the oxidation of aromatic amines, and one of the first fruits was the discovery of Magenta by Verguin in 1859. This dyestuff was manufactured by Perkin and Sons using the mercuric nitrate process (Knosp, 1859; E. P., T. D., Perkin). A range of other dyestuffs of the basic type manufactured included Dahlia, made by the action of ethyl iodide on Mauve, Safranine (Grenville Williams, 1859), and Britannia Violets, made by heating Magenta in methanolic solution with brominated turpentine under pressure. These dyes had a range of shades, and a blue variety was partly converted into the successful Perkin's Green by heating with acetyl chloride. In this connection the manufacture of phosphorus trichloride, and of acetyl chloride, was first carried out.

A development of importance almost equal to that of the triphenylmethane and azine dyes was the production of artificial alizarine. This colouring matter of madder had been regarded as related to naphthalene, and only in 1868 was it recognized by Graebe and Liebermann to be a derivative of anthracene. A year later they disclosed a synthesis—the first of a complex natural colouring matter—based upon fusing a dibromoanthraquinone with potassium hydroxide. As Perkin's earliest work under Hofmann's guidance had been concerned with the isolation of anthracene from coal-tar and with the preparation of anthraquinone and chloro-anthracene, he was in a good position to attack the problem of alizarine synthesis on an industrial scale. The synthesis of phenol from benzenesulphonic acid by alkali fusion was discovered by Kekulé and Wurtz in 1867 and Perkin decided to try and make anthraquinonedisulphonic acid in the hope of applying

the same reaction to the formation of alizarine. The patent describing the successful development of this idea was one day later in date than a similar one taken out in Germany by Caro, Graebe, and Liebermann. An agreement was negotiated by which the English rights were allotted to Perkin and Sons. Not very much later, in 1869, Perkin superseded his earlier method by one dependent on the sulphonation of 9:10-dichloroanthracene with formation of anthraquinonedisulphonic acid. The 'alizarine' made by the first method contained anthrapurpurin and flavopurpurin, whereas that obtained by the new process was more correctly described as anthrapurpurin (1:2:7-trihydroxyanthraquinone) containing some alizarine and some flavopurpurin.



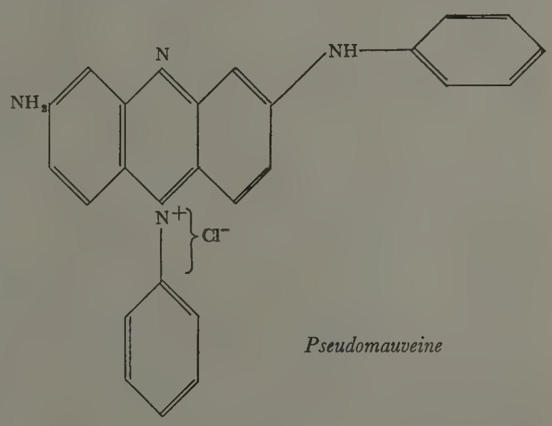

Only much later was it realized that the alkali fusion involved oxidation, that anthraquinonemonosulphonic acid is the intermediate for alizarine, and that the yield can be increased by the addition of an oxidizing agent (nitrate, chlorate) to the melt.

In 1869 Perkin and Sons made 1 ton of alizarine paste, and in 1873, 435 tons. But in 1874, the Greenford Green works was sold by Perkin to Messrs Brooke, Simpson, and Spiller, who later transferred it to Messrs Burt, Bolton, and Haywood. This firm moved the alizarine manufacture to Silvertown, where the enterprise became The British Alizarine Co. Perkin was thus the prime mover in founding two quite distinct branches of the artificial colour industry, and it should be added that he also introduced the system of technical service to dyers and other customers which later became such a powerful weapon in the hands of the German manufacturers.

After his retirement from business at the age of 36 Perkin devoted himself to pure research. A few of his contributions to pure science were the following.

In 1858 he effected, with Duppa, the first synthesis of glycine, by heating a solution of bromoacetic acid in aqueous ammonia. The same authors cleared up many aspects of the relation between maleic and fumaric acids and the isomeric tartaric acids. They could not, of course, at that stage of the development of the subject, reach a full understanding of the problems involved. The synthesis of DL-tartaric acid from dibromosuccinic acid was due to the same workers.

Mauve was separated by Perkin into three constituents and he identified the bases in his starting material which gave rise to each. Though a general idea of their structure was reached, the constitution of one of these substances was first settled by the synthesis of Perkin's pseudomauveine by O. Fisher and Hepp in 1888. This was the component derived from aniline only and the synthesis proved that it had the annexed structure.




*Pseudomauveine*

By reducing amino-azonaphthalene, Perkin and Church made the first diaminonaphthalene and discovered the method of degradation of azo-dyes which has been used ever since as a standard analytical procedure. Later Perkin used amino-azonaphthalene for the formation of Magdala Red by a greatly improved method.

A quite different field was opened up by Perkin's synthesis of coumarin from the sodium salt of salicylaldehyde and acetic anhydride (1868). This was followed up by the general Perkin reaction, the synthesis of cinnamic acid and derivatives, in 1877. This sequence recalls the similar example of Graebe's discovery of Alizarine Blue, later developed by Skraup as a general method of synthesis of quinoline derivatives.

From 1881, Perkin devoted most of his attention to a study of the effect of a magnetic field on the optical rotatory power of organic substances. He related the magnitude of the effect to structure in a masterly manner and showed that his results ran parallel to those obtained in a similar study of refractive and dispersive powers. His last paper (1907) was on the magnetic rotation and refractive power of hexatriene and the relationship of this substance to benzene.

## WILLIAM HENRY PERKIN, JUN.

In 1859 Perkin married his cousin, Jemima Harriet Lissett, who was of Huguenot descent. Their first-born was William Henry (1860), followed (1861) by Arthur George. Mrs. Perkin died shortly afterwards, a victim of tuberculosis. Six years later Perkin married Alexandra Caroline Mollwo, by whom he had a son, Frederick Mollwo, and four daughters.

The financial success of the works at Greenford Green and the early stage at which Perkin left his great work to be carried on by others, together with his preoccupation with scientific work, doubtless smoothed the path for his eldest son and indicated a career in pure science. But the chief contributory factor to the fortunate outcome was the chemical talent, amounting to genius, which William Henry Perkin, jun., showed that he possessed from the time when he first touched a test-tube. He studied for a time in Frankland's laboratory at the Royal College of Chemistry (1877), which had been transferred from Oxford Street to South Kensington. Here he carried out some researches under W. R. E. Hodgkinson. One of these, 'On the action of sodium on phenylic acetate', published in the Journal of the Chemical Society, will be recognized as truly Perkinian by the cognoscenti.

In the house at Sudbury in which the Perkins then resided there was a laboratory in a building in the garden, and both the sons worked there in the vacations. So far as the writer is aware the only recorded collaboration between the brothers was a study of the pyrolysis of silver salt (sodium anthraquinone-β-sulphonate), and of other derivatives of anthraquinone; this may have been merely the result of propinquity, though the work was published later, in 1885 and 1888. It is strange that the young chemists overlooked the fact that the red product, which was in fact di-anthraquinonyl sulphide, contained sulphur!

In 1880, William went to Würzburg and studied under Wislicenus, though not without some serious opposition from his father, who feared that in Germany his son would be too much exposed to the influence of free-thinkers. Perkin has given an eloquent tribute to Wislicenus in a Memorial Lecture delivered to the Chemical Society. However, after a brief interlude in his father's laboratory, he joined the brilliant group that clustered round Adolf von Baeyer at Munich. He became an ardent disciple, and later a close friend, of that great man, to whom for a long period he made an annual visit. Baeyer set him to work to study the reactions of benzoylacetic acid and its esters. At that time the elegant method of Claisen was not known and the acid had to be made by reacting phenylpropiolic acid with sulphuric acid and later with water:

$$Ph.C \equiv C.CO_2H + H_2O \rightarrow Ph.CO.CH_2.CO_2H$$

The acetylenic acid was made from cinnamic acid dibromide by the action of alcoholic potash, and the yield was rather poor in the last stage. Baeyer, in his round of the laboratory, was astonished by the amount of benzoylacetic ester that his new pupil had accumulated and began to read him a lecture on undue prodigality in the use of material. But Perkin had discovered a better method, and on this being explained Baeyer's reported comment (from W.H.P. to the writer) was, 'I see you are not so great a fool as I took you to be'.

Perkin became a privatdozent at Munich and took full advantage of his exceptional opportunities not only for communion with kindred spirits, such as Königs, Bamberger, Curtius, von Pechmann, Friedländer, O. Fischer, and many others, but also for participation in the typical life of the University and the enjoyment and practice of music. It should be noted that all the Perkins were musicians and there was a family orchestra of almost professional calibre.

It was here that he started one of his most original and far-reaching researches, that on the synthesis of substances whose molecules contained carbon rings, even those of three or four atoms.

FIGURE 1 – *Sir William Perkin, from a painting by A. S. Cope in the National Portrait Gallery, London.*

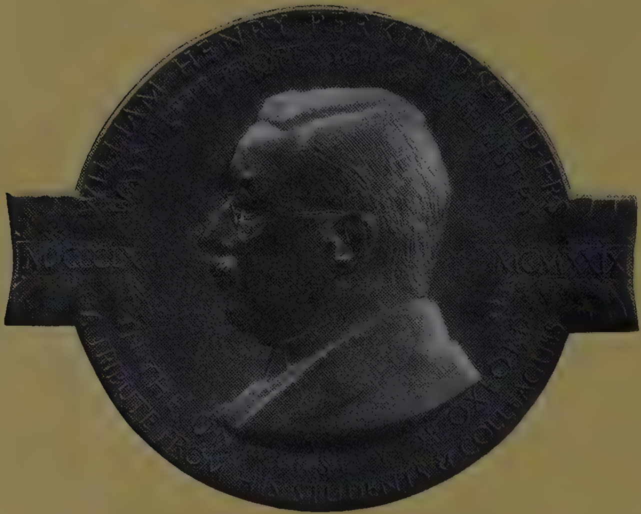


FIGURE 2 – *W. H. Perkin, jun., from a memorial plaque in the Dyson Perrins Laboratory, Oxford.*

FIGURE 3 – *W. H. Perkin, jun.*

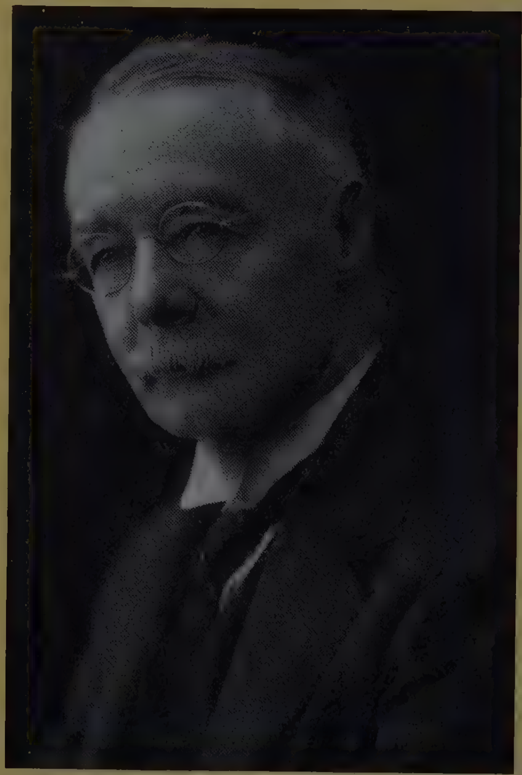


FIGURE 4 – *A. G. Perkin.*

Victor Meyer frequently visited Munich and on one occasion had a long discussion with Perkin in regard to his plans for this investigation. Meyer deprecated the idea, because he thought that only five-membered and six-membered rings could exist; at least there was then no evidence that other types occurred in natural products or had been obtained in synthetical work. Emil Fischer and Baeyer himself joined their voices to that of Meyer. Their argument was certainly valid at the time, but Perkin quickly proved that it was nevertheless fallacious, because he found several methods for the production of the smaller carbon rings, as well as for those of the more usual types. This discussion of plans for a research strikes no familiar chord nowadays. The standards of courtesy have sadly deteriorated, and in place of consultation we find a Provisional Patent in the form of a Preliminary Note: even this is often infringed.

At a little later stage he became involved in a controversy with Fittig, who threw doubt on the correctness of Perkin's view of the nature of his *cyclo*-propanecarboxylic acid (trimethylenecarboxylic acid); Fittig thought it was probably vinylacetic acid. However, the synthesis of the latter by J. Wislicenus settled the matter in Perkin's favour. The ramifications of this work cannot be followed in such a brief account as this; a much later development was the work on camphor and the syntheses of limonene, sylvestrene, and the terpineols, the latter being *Analogie-Arbeiten*, but of the most elegant kind.

As a result of a meeting with H. B. Dixon at Montreal, during the Canadian meeting of the British Association for the Advancement of Science in 1884, he returned from Munich to Manchester for a short period and there started his important studies on brazilin and haematoxylin. The work did not progress far at that time, but was taken up again on the return to Manchester in 1892.

After failing to secure the Chair at Aberdeen (F. R. Japp was appointed and thoroughly deserved and justified his success), he became Professor of Chemistry at the Heriot Watt College in Edinburgh and five years later accepted the invitation to fill the Chair of Organic Chemistry at Owens College rendered vacant by the death of Carl Schorlemmer. Here he developed a remarkable school of research and was influential in promoting extensions of the facilities, making plans for new buildings which were, however, carried out only in part during his tenure of the Chair.

In 1912 he was elected Waynflete Professor of Chemistry, Oxford University, and Fellow of Magdalen College. There he continued with unabated vigour his brilliant researches, attracted eager pupils from many parts of the world, and left his mark on the University and its chemical school; it suffices to mention the Dyson Perrins Laboratory and the Part II of the Honour School.

If a more complete account of Perkin's work is desired, the reader may be referred to the considerable Obituary Notice published by the Chemical Society (1932), where it will be found summarized in over 100 pages. But in addition to his varied synthetical work, two aspects of his research activity must be specially mentioned here.

Goldschmiedt, over a period of years from 1883, was the first to attack the constitution of a natural product, in this case papaverine, by oxidation of the substance with potassium permanganate, so as to form a variety of products the nature of which could be determined more easily at least than that of the progenitor. These simple molecules were thus the pieces which could be fitted together, often overlapping, so as to reconstitute the original, as in a jig-saw puzzle.

At Edinburgh and Manchester, and with some projection to Oxford, Perkin applied the idea with conspicuous success to the complex alkaloid berberine and to brazilin and haematoxylin, in the latter cases using methyl ethers. In his exhaustive study of cryptopine, and again in his work on strychnine and brucine, the method was somewhat different and consisted in a study of all the transformations which he could effect of the complex molecules. Perkin modelled himself on Baeyer and developed to an extraordinary degree the qualities which he most admired in the master. At all times he worked in the laboratory most assiduously with his own hands and had a wonderful, uncanny instinct for the selection of the best methods for the separation of the constituents of tars and suchlike unpromising material.

He had little interest in theoretical chemistry. For him chemistry was an art or craft designed to embellish the edifice of molecular structural knowledge. This was erected largely during his lifetime, and he made great additions to it himself. He was, however, quick to adopt new techniques when he was sure of their value; for example, he became very skilled in high-vacuum distillation.

As a lecturer, and teacher generally, he was extremely successful, and his fame in this respect is recognized not merely by the organic chemists who were fortunate enough to collaborate with him in original work, but also by the larger number

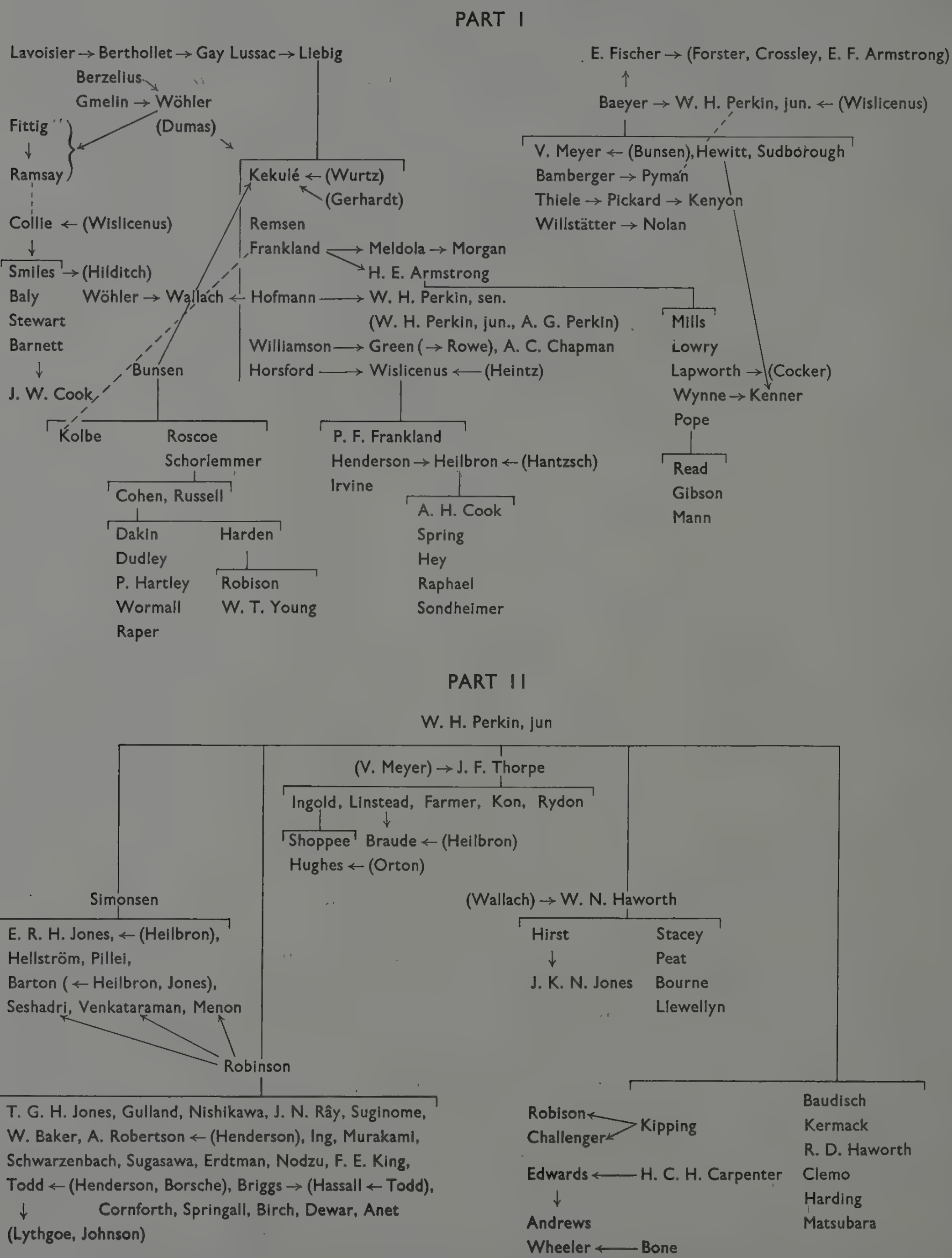



FIGURE 5 – *Academic associations of some organic chemists, primarily designed to illustrate the far-reaching influence of W. H. Perkin, jun.*

of undergraduates who attended his lectures and retained the most vivid memories of his highly characteristic lucidity of exposition.

A favourite expression was 'Let us put it down in the form of a little table': accordingly the writer has thought it appropriate to show in a similar fashion the profound influence, or at least a part of it, of W. H. Perkin, jun., on organic chemistry in Britain.

The annexed table (figure 5) illustrates, although in a manner certainly incomplete and probably in some respects fallacious, the academic relations of some British chemists. The chief object has been to show the influence of W. H. Perkin, jun., but to do this in a fully satisfactory manner would fill several pages: hence directors of research, and others engaged in technical, administrative, or quasi-confidential work, have not been mentioned. Those who have occupied chairs in universities or have been admitted into the Fellowship of the Royal Society are included, but even so there will be omissions due to the limited knowledge of the author.

### ARTHUR GEORGE PERKIN

Arthur George Perkin was a weak infant and was not expected to live. At the age of six he went to a boarding school at Margate and when ten years of age he joined William at the City of London School. Hence his home life was confined to vacations, when the chief occupation of both boys was to carry out chemical experiments in the garden laboratory, where there was a bench for each under opposite windows. At the age of seventeen he entered the Royal College of Chemistry and studied under Frankland and Guthrie. He did some original work and published a paper on 'The Action of Nitric Acid on Di-*p*-tolylguanidine' in 1880.

When William went to Würzburg, Arthur spent a year at Anderson's College, Glasgow, and then won a scholarship, newly instituted by the Worshipful Company of Clothworkers, tenable in the Dyeing Department of the Yorkshire College, later the University of Leeds.

Here he worked with J. J. Hummel, for whom he always professed the greatest admiration, and this contact orientated his chemical interest in the direction of the chemistry of natural dyestuffs. This subject he made his own, and his contributions in the field gave him international fame and permanent recognition as a pioneer. One piece of joint work with Hummel concerned the action of strong acids, for example, sulphuric acid, on brazilein and haematein. The so-called isobrazilein and isohaematein salts thus produced were the first pyrylium derivatives to be isolated and analysed, though the true nature of the substances could not be understood in those early days. On the expiry of his scholarship, Perkin accepted an appointment as chemist in the firm of Hardman and Holden of Manchester. He was doubtless influenced in the decision to take up this work by the nature of their business, which was concerned with the manufacture of artificial alizarine. In 1888 he became manager of the works.

In 1892 he went back to the Yorkshire College as lecturer with research duties. The opportunity was seized to the full, and for a long period of years he worked assiduously day, and sometimes night, often assisted by his wife (*née* Bedford), studying the constituents of dyewoods and other sources of natural colouring matters. He was especially interested in the naturally occurring dyestuffs, and this directed his steps towards the anthoxanthins and above all to the flavonoid types.

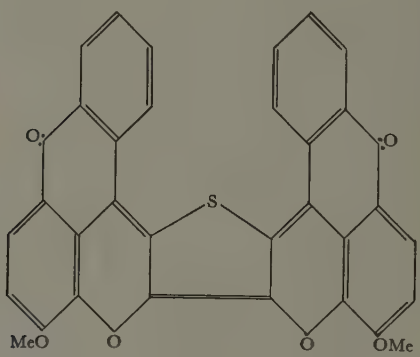
In 1916, Perkin succeeded A. G. Green as Professor of Colour Chemistry and Dyeing in the University of Leeds. His teaching and administrative duties gradually became more onerous, and matters were not improved by the extreme conscientiousness which was characteristic of him. The writer can give an instance of this from his own experience: acting occasionally as his External Examiner it was possible only to admire, with what patience could be summoned, Perkin's practice of reading aloud every word of every script.

According to F. M. Rowe, his successor in the chair, this increase of routine led him in later years to devote his attention to less arduous studies. Nevertheless he worked constantly and personally to the end of his life and the investigations of the later years do not show any faltering of one of the most skilful pair of hands which has served our science.

He retired in 1926 and died in 1937. Although he had many collaborators, both British and from abroad, about 60 of his 270 memoirs were published in his own name alone. He was indeed essentially an individual worker, happiest with a single objective at any one time and not possessing in a comparable measure his brother's flair for the organization of research on a wide front. Throughout his life Perkin worked on derivatives of anthraquinone, particularly the hydroxylated substances of natural origin. He paid special attention to the

partly methylated di- and trihydric phenols of the group and devised methods for determining their structure and for their synthesis.

From 1922 to 1933 he examined the reduction products of the hydroxy-anthraquinones, determined the structure of the products, and showed how they could be condensed into bis-anthracene derivatives including naphthadianthrones and helianthrones. His very last work, with N. H. Haddock, arose from this topic. In attempting the methylation of 3:4-dihydroxyanthranol, a green vat dye was obtained and the formation of the substance was found to be due to an impurity in the trichlorobenzene used as the reaction medium. This was identified as tetrachlorothiophene, and the probable constitution of the colouring matter is as shown below.




The kernel of Perkin's scientific work was a long series of researches on flavones and flavonols of natural origin.

The natural yellow dye weld (from Dyers' Weed, *Reseda luteola*) was known to the Gauls in the time of Julius Caesar. Its colouring principle was first studied by Chevreul and later by other workers, but nothing was known in regard to the structure of its molecules. Perkin improved the method of isolation of this luteolin and showed that it is a tetrahydroxyflavone based on phloroglucinol and catechol. Many years after luteolin had been generally abandoned, the silk facings of military uniforms were dyed by means of its bright yellow aluminium lake. Similarly mauve, otherwise long superseded, was used to colour the lilac penny postage stamp of Queen Victoria's reign.

Perkin was not the first to recognize the nature of the flavonoid colouring matters. Earlier work by Herzig (fisetin) and Kostanecki, and Piccard (chrysin) laid the foundations, but because of his great extensions of the group he must be ranked with the above-named pioneers. It suffices to mention apigenin (first isolated by von Gerichten), acacetin, morin, myricetin, quercetagetin, gossypetin, rhamnetin, *iso*rhamnetin, among many others. Perkin also encountered many substances allied to the flavones, such as butin and butein (from *Butea frondosa*) and in addition studied xanthones, such as gentisin. He garnered the information which later led to the recognition of the existence of the isoflavones.

A brand plucked from the burning was carajurin, once used as a cosmetic pigment by the natives of the Upper Orinoco. Perkin obtained a specimen from a curio dealer in London, but the secret of its preparation appears now to be lost. The material afforded two crystalline substances, of which the more abundant was the deep red carajurin. The writer was privileged to take part in the determination of the structure of carajurin and it has recently been synthesized by Seshadri. Incidentally it should be mentioned that Indian and Japanese chemists have greatly enlarged our conceptions of the importance of the flavonoids, and the influence of A. G. Perkin on the direction taken by their work is easily discerned.

Perkin also made significant contributions to our knowledge of the catechins and discovered the curious substance cyanomaclurin, which has the composition and structure of a reduced morin, intermediate in state of oxidation between a catechin and a flavonol. Perkin's strong suit was the accumulation of 'know-how' for handling these and other natural products.

## FREDERICK MOLLWO PERKIN

The third son of Sir William Perkin, Frederick Mollwo, collaborated with Arthur in some work on the production of purpurogallin. He wrote a textbook on qualitative analysis and one recalls with gratitude that the composition of certain named reagents, such as Mandelin's, was given therein. This is a matter which is often obscure.

As in the case of the important chemical technologists who owed so much to the early inspiration given by W. H. Perkin, jun., it is impossible to assess the value of confidential work by reference to detail. It is clear, nevertheless, that the overall effect has been important. This consideration applies to Perkin's third son, whose success as a technical consultant showed that he inherited one aspect of Sir William's genius, and that, as we have tried to show, by no means the least important aspect.

# Book reviews

### DICTIONARY OF SCIENCE

Kosmos Lexikon der Naturwissenschaften, Volume 2, L–Z. *Pp.* 904. *Kosmos Gesellschaft der Naturfreunde; Frankh'sche Verlagshandlung, Stuttgart.* 1955. *Cloth, DM.* 35.50; *Leather, DM.* 42 *net.*

The first volume of this dictionary was reviewed in *Endeavour* in April 1954: the second volume completes the work, which can now be viewed as a whole. More than 25,000 terms, covering many branches of science, have been compiled and explained by specialists; among the biological sciences, which are specially stressed, zoology seems to have been particularly favoured, as the list of contributors shows nine zoologists as compared to one from each of the other branches of science dealt with.

As a reference book for non-scientists interested in the impact of science on the modern world this book will serve a very useful purpose, and the literature cited is in many instances up to date and will stimulate further reading. It seems, however, that the fact that each reference was dealt with by a specialist has led to a certain neglect of those fields of research where two or more branches of science overlap, such as biochemistry and biophysics, though these also might be of interest to a general reader.

The table on the development of mankind (columns 1863–65) might appear rather bewildering to the layman, and the survey of the history of the sciences contained in ten columns cannot be of real value, even to a non-scientist.

Seen as a whole, the dictionary is an admirable achievement, and with its clear drawings and delightful coloured plates will certainly become one of the most popular of the Kosmos publications.

G. SCHLESINGER

### LOGOTECHNICS

Composition of Scientific Words, *by Roland Wilbur Brown. Pp.* 882. *Published by the author, from the U.S. National Museum, Washington.* 1954. $8 *net.*

Because of the need for a concise way of denoting new and complex ideas the sciences have been prolific begetters of new words, many of which have come into general usage. Most are derived from Latin and Greek roots, but the decreasing attention paid to the classical languages in the early education of scientists has, among other causes, led to many unsatisfactory developments in nomenclature. The author of the present book—a greatly enlarged version of his 'Materials for Word-Study' (1927)—has made a lifelong hobby of the practical application of Latin and Greek in the formation of literary and scientific terms. All those who are interested in the writing of good English must be profoundly grateful to him for putting the results of his studies permanently on record. The book's purpose is twofold. On the one hand to mitigate some of the ills of word composition: on the other to discourage the making of unnecessary words and the prolification of jargon.

Although the bulk of the book consists of a cross-reference lexicon of words and roots, there are also excellent introductory sections which include discussion of the nature of Latin and Greek and rules for deriving scientific terms from them. While the weight of emphasis is naturally on the classical languages, the host of others—from Sanskrit and Polynesian to Anglo-Saxon and aboriginal American—which have contributed to the richness of English receive due mention.

But one word of warning must be offered to the intending user. Unless his firmness of purpose far exceeds that of the present reviewer he will find much time slipping pleasantly away in perusing entries far removed from those he originally intended to seek. Time so lost will not by any means be wasted, however, for any amount of browsing in this book will be profitable.

TREVOR I. WILLIAMS

### A SYSTEM OF NATURE

The Unified System Concept of Nature, *by S. T. Bornemisza. Pp. viii* + 137. *Vantage Press Inc., New York.* 1955. $3 *net.*

This is an unusual kind of book. The author seeks to arrive at a general view about living and non-living systems in nature, their ordering into hierarchies of different complexity, and their interrelations. He begins by contrasting recurrent or cyclic changes with discontinuous structural changes, and distinguishes what he calls the internal and the external relations of systems, pointing out how the external relations of one set may be the internal relations of a set of different order. In this general connection the principle of entropy compensation becomes very important. Biological systems are then considered, and a doctrine of what is called the 'organic image' is propounded according to which 'living organic matter harbours the reproduction of its environment'. This organic image contains elements not only of the past, as in memory, but of the future, in that it determines heredity.

The latter part of the book tackles the question of psychophysical parallelism. The author seems to subscribe to the form of doctrine which finds some element of the living in everything.

Generally speaking, the book is based on sound scientific principles. The philosophical discussion at times strikes the reader as illuminating and even eloquent, and at others nearly as an embroidery of the obvious. The talk about organic image may seem to some a little far-fetched, but it has at least many elements of truth. Some of the attempts at analysis of the relation between the physical and psychological are necessarily very speculative, but on reflection one may feel that they are not more surprising than much that is the common currency of psychoanalysis.

C. HINSHELWOOD

### THE RENAISSANCE AND EARLY SCIENCE

The Appreciation of Ancient and Medieval Science during the Renaissance (1450–1600), *by George Sarton. Pp. xvii* + 233. *University of Pennsylvania Press, Philadelphia; Geoffrey Cumberlege, London.* 1955. $5 *net.*

If for no other reason than the prodigious mass of his published writings, George Sarton would be an outstanding figure in the world of learning. But voluminous output in itself is no criterion of merit. Sarton's distinction is that his colossal achievements—his 'Introduction to the History of Science' alone runs to some 5000 closely printed quarto pages—maintain throughout a very high standard of careful scholarship. The present book is no exception. It deals with Renaissance appreciation of ancient and medieval medicine, natural history, mathematics, and astronomy in the period 1460–1600.

An important point is made early in the book, namely that the flood of Renaissance editions of old medical and scientific treatises was not inspired by curiosity or disinterested scholarship,

but by utilitarian ends. In general, the reputation of ancient and medieval authorities was so high that more attention was paid to the establishment of accurate texts than to consideration and criticism of their subject-matter. The typical Renaissance approach to science was in fact 'a literary and wordy approach instead of being experimental', though there were a few bold and independent spirits such as Palissy, Paracelsus, and Petrus Severinus.

In the medical section, Sarton deals with the reception of the works of Hippocrates, Celsus, Soranos, Galen, the early Byzantine physicians, and Avicenna, the last named retaining his position of authority in spite of an anti-Oriental spirit shown by medical humanists of the time. In the section on natural historians, Aristotle, Theophrastus, Dioscorides, and Pliny receive most attention, while in that on mathematics and astronomy the principal names, as we should expect, are those of Plato, Aristotle, Euclid, Archimedes, Apollonius of Perga, and Ptolemy.

The book is fully documented, with about forty pages of notes, and there is a good index. The text is bursting with information and enlivened from place to place with characteristically Sartonian comments and judgments.

E. J. HOLMYARD

### HISTORY OF SCIENCE

An Illustrated History of Science, *by F. Sherwood Taylor, illustrated by A. R. Thomson. Pp. xii + 178. William Heinemann Limited, London.* 1955. 25*s. net.*

This is a most attractive book—one that scientists will buy for presentation and then find they cannot part with. But its appeal will reach a much wider public than a purely scientific one: it is so delightfully written and illustrated that everybody interested in the history of what has become the major factor in modern civilization may well find the book irresistible. The text, which is based on the Christmas lectures given by the late Dr Sherwood Taylor at the Royal Institution in 1952, outlines the history of science from the earliest times to the present day: not as a *tour de force* of desiccated compression but as a lively narrative centring round the fundamental experiments of natural philosophy. At the lectures themselves, these experiments were demonstrated as nearly as possible in their original fashion, and to compensate for the loss of this visual aid the author had the happy idea of illustrating the book with realistic pictures. He was taking a big risk, for there have been many awful warnings of the grotesque distortions inflicted by artists on scientific history. Fortunately the risk was justified, for Mr Thomson's drawings interpret their subjects with perceptive skill and are wholly admirable: those of Leeuwenhoek and Lavoisier deserve special mention, but the standard throughout is excellent.

This combination of a readable and authoritative text with illustrations sympathetically conceived and executed makes the book something novel in the literature of the history of science: it certainly deserves a great success and will doubtless achieve it.

E. J. HOLMYARD

### MARS

Physics of the Planet Mars, *by Gerard de Vaucouleurs. Pp.* 365. *Faber and Faber Limited, London.* 1954. 50*s. net.*

Mars is of special interest because it is the only planet possessing an atmosphere whose surface can be seen, and because changes are observed to occur in the delineation of the greenish surface markings, which can plausibly be accounted for on the hypothesis that these areas are covered with vegetation akin to the lichens that grow on the Earth. In addition, the seasonal growth and disappearance of the polar caps and the frequent cloud formations provide evidence of water on Mars.

This book contains a very complete account of observations on the physics of Mars published during the last forty years or so, together with a critical analysis of them. It is divided into five parts. Part I deals with the atmosphere of Mars, its composition, structure, and pressure, and the cloud formations. Part II deals with Martian climatology and temperature; Part III with the polar caps and water on Mars; and Part IV with the surface details and their interpretation. The internal constitution of Mars is discussed in Part V, which is followed by a general summary and conclusion. A comprehensive bibliographical list of references and subject and name indexes complete the volume.

Such a comprehensive account of Martian phenomena has never previously been published. It is a valuable source of information on all matters relating to Mars and will be most useful for reference purposes.

The least satisfactory part of the book is the reproduction of various photographs of Mars, which are of very poor quality and fail to show much of the detail shown in the originals.

H. SPENCER JONES

### THE POLAR AURORA

The Polar Aurora, *by Carl Störmer. Pp.* 437. *Oxford University Press, London.* 1955. 55*s. net.*

The aurora has close connections with important types of ionospheric irregularity, so that a book on this subject finds an appropriate place in this series of international monographs on radio. The most authoritative voice in the field of aurora during the past fifty years has been that of Carl Störmer; it is a considerable achievement on the part of the editors of this series to have persuaded Professor Störmer to write this book, and a great achievement on the part of the author, who is now over 80 years old, to have brought the book to a successful conclusion.

Observation and theory are considered in about equal measure. Observational work is first dealt with in detail—auroral forms, and visual and photographic measurement of height and position and of spectrum measurement. Among other items considered are the rather inconclusive results so far obtained in various attempts to use radar methods in auroral study. Some auroral statistics are given, though in general this is the aspect of the subject to which the author gives relatively least weight.

Störmer's classical work on the trajectories of electric corpuscles in a magnetic field naturally dominates the second part of the book. In a separate chapter he describes the application of this theory to cosmic rays. The author discusses in detail those features of aurora which the theory can and cannot explain, and refers to the alternative theories of Chapman and Ferraro, Martyn, Alfvén, and others. The complexities of the aurora are such that the author foresees a complete solution only in the distant future.

This book will undoubtedly be for many years the standard reference book on the aurora. Its publication is timely, as it will give many auroral workers intending to take part in the International Geophysical Year a ready means of familiarizing themselves with the subject.

D. H. McINTOSH

### FRONTIERS OF ASTRONOMY

Frontiers of Astronomy, *by Fred Hoyle. Pp.* *xvi* + 360. *William Heinemann Limited, London.* 1955. 25*s. net.*

Forty years ago the question whether the objects known as spiral nebulae were members of our Galaxy or were extra-galactic objects was still undecided. Modern large telescopes have provided the answer, and information has been obtained about the distances, sizes, spatial abundance, and line-of-sight motions of these objects, leading to the conception of the expansion of the Universe, the interpretation of which is not at all clear.

Coming nearer home, there is much that is still not yet understood; for instance, the nature of the interior of the Earth, the cause of the ice ages, the origin of the lunar features, the structure of the planets, the origin of the solar system, the nature of cosmic rays. On all these matters there are new and interesting theories. The discovery by Baade by the two different stellar populations has given a new orientation to many fields of stellar investigation, while views about stellar evolution appear to be taking definite shape.

Hoyle has attempted the big task of giving an account of all the various developments which are the growing points in astronomy. Commencing with the Earth, he goes outwards to the Moon, the planets, the Sun, and the individual stars. Our own Galaxy is then considered, leading to galaxies in general, their formation and evolution. Finally he discusses the expansion of the Universe and the alternative explanations of the initial explosion theory and the steady-state theory, the latter entailing the hypothesis of the continuous creation of matter.

There is much, of course, that is still uncertain, and where differing views are held they are presented fairly, though not unnaturally it is sometimes clear where the author's preference lies. The book is intensely stimulating and should be read by all who wish to learn about the current trends in astronomical thought.

H. SPENCER JONES

### WOLFGANG GAEDE

Wolfgang Gaede, *by Hannah Gaede. Pp. viii* + 127. *Verlag G. Braun, Karlsruhe.* 1955. DM. 5.80 *net.*

The creation of a vacuum, generally a high vacuum, is a necessary condition for practically all experimental research concerned in modern investigations of the ultimate structure of matter and is fundamental for what may be called the light electrical industry and for nuclear power. The modern vacuum pump, as distinct from the old cylinder pump or the tedious hand-operated mercury pump, is the creation of Wolfgang Gaede. From the time when, at the age of twenty-seven, he designed his rotary mercury pump and, a little later, his box pump, he originated a series of pumps on new principles, in particular the molecular pump and the diffusion pump, based, in all instances, upon deep theoretical knowledge, clear physical insight, and a genius for design.

The present book is by his devoted younger sister, who was his playmate in childhood and his confidante and companion throughout his life, for he never married. It gives an account of his life from his birth at Freiburg in 1878 to his death in 1945 and, naturally, has much to say of the work which was his passion. He apparently sought neither fame nor riches, but only to pursue the discoveries in which his great powers found expression. He was, however, no recluse: he loved walking and sailing small boats with others, and was an accomplished violinist.

The last years of Gaede's life make a sad story. He lost all the money that he had made by his inventions in the catastrophic inflation in Germany of 1923, but became an inspiring university professor and with great success continued his work on the production and measurement of high vacua. In 1933, the year in which he was awarded the Duddell Medal in London and the Siemens Ring in Berlin, he was denounced by Nazi personalities as politically unreliable and friendly to Jews, and was removed from his university post—he was, indeed, threatened with prosecution. Of his examination by the political powers he said that at the time he had the feeling '*entweder sei er verrückt geworden oder die anderen seien verrückt.*' He was shamefully treated but able to continue his work in a private laboratory, which was totally destroyed by bombs in 1944. In the following year he died of diphtheria and undernourishment.

The book is beautifully written and gives a vivid account of life in cultivated German circles before the end of the old European civilization in 1914, and an intimate picture of a rare inventive genius and a man of great character.

E. N. DA C. ANDRADE

### PHYSICS AND MICROPHYSICS

Physics and Microphysics, *by Louis de Broglie, translated by M. Davidson. Pp.* 286. *Hutchinson's Scientific and Technical Publications, London.* 1955. 21*s. net.*

Louis de Broglie, the founder of wave mechanics, is a lucid and inspiring speaker on themes concerned with physics as a branch of learning rather than as a branch of technology, and the present book is a collection of lectures and expositions, some seventeen in number, mostly on such themes. They were, clearly, delivered to different types of audience at different periods, at least one before the invention of the atomic bomb; it would have been of interest to know the date and occasion of each address, which is given in only two cases.

The title refers to the physics of ponderable bodies, in the ordinary sense, which is often called classical physics, where continuity and certainty reign, and to the physics of ultimate particles and wave packets, where quantum theory and uncertainty prevail. Most of the book, however, deals, as might be expected, with microphysics, in particular with matters in which wave mechanics plays a prominent part, physics being here that which has been superseded. Thus the first chapter, evidently written more than ten years ago, deals chiefly with the wave mechanical theory of the electronic structure of the atom in simple style, and the second chapter with much the same subjects in rather more detail. There is, in another chapter, a somewhat more technical treatment of the formation of images in corpuscular optics, bearing on the possibilities of the electron microscope and leading to the conclusion that we can never see, in the microscopic sense, the structure of atomic edifices. These chapters, with a few shorter addresses on related subjects, form the first part of the book.

A second part, labelled not altogether appropriately 'scientific philosophy', contains the longest and most personal articles. One, entitled 'Revelations of Microphysics', deals with the fundamental implications of the quantum theory, in particular with the uncertainty principles and the disappearance of the individuality of the corpuscle, with its bearing on determinism. Another begins with the author's personal memories of the start of wave mechanics and then proceeds to consider the problems of the nuclear field, discussing very difficult subjects with a masterly simplicity of style. The

chapter most nearly approaching academic philosophy is one on Bergson's conceptions, which are presented in abbreviated form, as are in most cases the less significant chapters on the history of the sciences that conclude the book. E. N. DA C. ANDRADE

### NUCLEAR SHELL STRUCTURE

Elementary Theory of Nuclear Shell Structure, *by M. G. Mayer and J. H. D. Jensen. Pp.* 269. *John Wiley and Sons Inc., New York; Chapman and Hall Limited, London.* 62*s. net.*

For many years it was believed that nothing existed in nuclear physics comparable with the essentially simple structure of the atom as reflected in the periodic system of the elements. By 1948, however, the accumulation of precise data indicated clearly that there does exist a nuclear shell structure very like a periodic system for nuclei. Independently, Mayer in America, and Haxel, Jensen, and Suess in Germany, found the explanation for it. Since then, the 'nuclear shell model' has had great success in correlating experimental material.

The appearance of a book by Mayer and Jensen, the two main originators of the theory, is therefore to be welcomed. In it the experimental evidence pointing to nuclear shell structure is lucidly marshalled, followed by a simple explanation in terms of nuclear orbitals, in which the coupling between spin and orbital motion in nuclear dynamics plays an important role; there is a detailed discussion of empirical nuclear systematics in the light of the theory. Beyond this the book is disappointing. It lacks a coherent plan of subject matter. The authors have attempted to say a little about many topics of nuclear physics. Often they seem unaware of important contributions to the subject. Indeed, the book is marred by a defect common to many publications from America, namely, ignoring almost all work which has not been published in the *Physical Review*, and even from that the omissions are many. Not only are references to original sources very incomplete, but the authors are careless about such details as the spellings of names and the definition of physical quantities. The printing of mathematical formulae is ungainly, and the quality of the illustrations is very mixed—some are very clear, others succeed only in confusing the point that should be illuminated. M. H. L. PRYCE

### NUCLEAR SCIENCE

Annual Review of Nuclear Science, Volumes III and IV. *Pp.* $x + 412$ *and* $x + 483$. *Annual Reviews Inc., Stanford, California, in co-operation with the National Research Council of the National Academy of Sciences.* 1953 *and* 1954. $7 *net per volume.*

It is one of the minor oddities of nuclear physics that of its two annual publications the 'Progress in Nuclear Physics' contains review articles, while the 'Annual Review of Nuclear Science' contains mainly progress reports. The policy of the new editors who are responsible for these two issues of the latter publication is that they should cover several years; in the first two issues many articles took rather too literally the idea of being an annual review. This is all to the good, for even a subject developing as rapidly as nuclear science does not lend itself to an annual stocktaking in every one of its branches.

The amount of research done in nuclear science today is remarkable, and the articles in these issues—few of which have fewer than fifty references and one has nearly six hundred—bear this out. But the quality of research varies, and many of the articles overwhelm the reader with an avalanche of facts, theories, and results without any attempt at their critical evaluation. Where such criticism is exercised and the work reported is set, however briefly, against the background of previous work, the articles are all that can be desired.

There are few branches of nuclear science that are not touched upon, and although the general production is excellent the volumes could be made even more informative to the non-specialist who likes to read an article without continually looking up references at the back by quoting references by author and year rather than by number. About half of each volume deals with nuclear physics, experimental and theoretical, and the rest is given over to nuclear chemistry, biology, medicine, and even geology. Here could be, and indeed often is, a means for broadening the scientist's mind. L. R. B. ELTON

### BOLTZMANN'S LAW

Boltzmann's Distribution Law, *by E. A. Guggenheim. Pp.* 61. *North-Holland Publishing Company, Amsterdam.* 1955. 5*s.* 6*d. net.*

It is a pleasure to review a book as short, elegant, useful, and inexpensive as this one. Many teachers of physics and chemistry must have regretted the fact that the simple bases of statistical thermodynamics are usually buried in a mass of mathematical detail, or arrived at only as an apparently irrelevant 'interpretation' at the end of an exposition of classical thermodynamics. Professor Guggenheim's book cuts right across the traditional line of approach by introducing Boltzmann's distribution law as an axiom for discrete energy states, thus defining temperature implicitly, and proceeding on this basis, expressions are derived for a number of bulk properties. No formal statement of thermodynamic laws is made, and entropy is not mentioned.

The topics discussed in detail include monatomic and diatomic gases, simple crystals, the dielectric constants of gases, and chemical equilibrium in gases. In every case the exposition is clear, important qualitative points are emphasized, and numerical values are given to impress upon the reader the orders of magnitude involved. Only an elementary knowledge of calculus is presupposed, and the preface describes the book as suitable for students in their first term at a university. It might be profitable to wait a little longer, so as to provide a basis of rather more experimental knowledge, but there is no doubt that the book can be read with profit at an early stage in a student's career, and our thanks are due to the author and to the publisher for producing so good an exposition at such a reasonable price. R. P. BELL

### NUCLEAR RADIATION DETECTORS

Nuclear Radiation Detectors, *by J. Sharpe. Pp.* 179. *Methuen and Company Limited, London.* 1955. 11*s.* 6*d. net.*

This little monograph is concerned with a very wide field, since it embraces within its scope almost the whole field of modern radiation detectors as applied in nuclear physics and allied subjects. Indeed, the main question that arises in reading the book is concerned with this point. Opinions will differ as to the wisdom or otherwise of choosing to condense the whole subject within a single monograph.

If the reader accepts the fact that space is definitely limited in treating the enormous amount of material available, then he can have little complaint

with the present volume. There is a veritable mine of information stored within its pages.

The subject matter is subdivided in a novel manner. It falls within five main chapter headings, namely the interaction of nuclear radiation with matter, detection media, efficiency of detectors, secondary emission and scintillation counters, and ionization devices. It is a logical arrangement of the subject and one that leads naturally to a step-by-step approach to the more complex devices. It is notable that scintillation counters precede ionization types—this is probably fully justified in view of the very wide versatility of the scintillation type as compared with the Geiger or proportional types. Many of the less universal counters are discussed, for example conduction counters and gridded ion-chambers receive attention.

As an introduction to a very important field the work can be highly recommended. Students should find it a profitable investment at its modest price. Those wishing or requiring more extended treatments will find that their needs are anticipated by the very extensive bibliography. S. C. CURRAN

### IONOGRAPHY

Ionography, *by H. J. McDonald. Pp. x* + 268. *The Year Book Publishers Inc., Chicago; Interscience Publishers Limited, London.* 1955. 48*s. net.*

Ionography is the term suggested by the author for the processes of electromigration of substances in media stabilized by paper, agar, or similar materials. Although not a new technique, the resurgence of interest in the method in the last few years has made it difficult for the newcomer to select from the multiplicity of designs the apparatus best suited to his needs, and details of specific applications have to be sought in an ever-widening range of journals. To help both the newcomer and the more experienced worker Professor McDonald and his collaborators have attempted to bring together in this book the available information on the theory, technique, and applications of the ionographic method. The emphasis is on paper-stabilized systems, although the use of other stabilizers is also reviewed. The result is a very readable account which undoubtedly owes much to the fact that the author is drawing largely on his own experience, particularly in the chapters dealing with mobility measurements. Pictures of actual separations have wisely been omitted, as they seldom gain in reproduction, but a number of well chosen diagrams and graphs illustrating specific points have been included. In the chapters dealing with applications the author and his collaborators have extracted the salient points from key papers and summarized them in a readable and informative manner. There is a very full bibliography citing papers up to 1954. One has the impression that much critical thought has gone into the preparation of this book, which should be of interest to all those wishing to make full use of this very powerful technique either for analytical purposes or for fundamental mobility studies.

A. J. WOIWOD

### ORGANIC SYNTHESES

Organic Syntheses, Collective Volume III, *edited by E. C. Horning. Pp. x* + 890. *Chapman and Hall Limited, London; John Wiley and Sons Inc., New York.* 1955. 120*s. net.*

Volumes of 'Organic Syntheses' are automatically consulted by the present generation of organic chemists whenever there is need to prepare in bulk in the laboratory a compound that cannot easily be purchased: consequently the latest addition to this series needs no introduction to its potential readers. The separate small volumes appear on library shelves with such regularity that it is hard to realise that it is now twelve years since Collective Volume II was published.

Volume III, in consequence, collects together publications of several years, and so the editor has taken care to correct and modernize the text, adding literature references up to 1952. Throughout the book can be found additional notes, giving valuable practical hints, submitted by those who have had occasion to repeat its preparations. In addition to the usual comprehensive indexes to the compounds described and to the reactions involved, the editor has added a further, most useful, index to the methods of preparation and purification of important organic solvents and of special inorganic reagents—as for instance ozone, sodamide, and active nickel, platinum, and palladium catalysts.

While everyone will be sure to regard some of the preparations as curiosities of little intrinsic value, nearly everyone will also find in this volume just that time-saving route to that crucially important substance which he himself needs, or at least the cognate preparation from which a required synthesis can speedily be developed.

W. A. WATERS

### CATALYSIS

Catalysis, Volume II: Fundamental Principles, Part 2, *edited by Paul H. Emmett. Pp. vi* + 473. *Reinhold Publishing Corporation, New York; Chapman and Hall Limited, London.* 1955. 94*s. net.*

This second volume of a lengthy cooperative work contains six chapters. The first is a valuable, concise account of very many reactions of gases and vapours, and the catalysts effective for them, discussed from the industrial point of view. The next deals with diffusion and reaction rates in the pores of catalysts, and suggests how pore structure might regulate what reactions are catalysed by porous substances. The next three chapters give brief accounts of some properties of solids—magnetic, electrical conducting and semi-conducting properties, the filling of electronic levels, and lattice dislocations—all of which might conceivably influence catalytic power but whose functions for catalysis, if any, are uncertain. Theories of reaction rate are given in some detail, but only a small part of them deals with reactions at surfaces. Adsorption is discussed at length in this volume, as in the first—even physical adsorption, which is admitted to have little to do with catalysis. There is, however, little detailed account of the structure of chemisorbed layers and still less of how such layers facilitate reactions which do not occur in their absence. There is, however, quite a good discussion of apparent variations in adsorptive power on different parts of a surface. These chapters give the impression of trying to mention everything which might conceivably have a connection with catalytic power, and to record many different suggestions, rather than to appraise these suggestions critically in relation to why catalysts work. A more selective and critical treatment might have been more illuminating, as well as less expensive to print; most readers are likely to emerge from these 170 pages with little enlightenment as to what general principles govern catalysis at surfaces.

The final chapter is more coherent, competently summarizing the theory of several types of homogeneous catalysed reactions; acid-base catalysis is particularly fully described.

Detailed treatment of several

important types of catalysis are promised for later volumes. May we hope that they will be more factual and less discursive, with theory (if any) more directly relevant to catalysis?

N. K. ADAM

ANALYTICAL CHEMISTRY

Analytical Chemistry—The Working Tools. *Edited by C. R. N. Strouts, J. H. Gilfillan, and H. N. Wilson.* Vol. I. *Pp. xxi* + 494; Vol. II. *Pp. ix* + 495–1067. *Oxford University Press, London.* 1955. £5 5*s. net for the two volumes.*

A foreword by the late Sir Wallace Akers states that these two volumes represent 'the collection of analytical techniques and methods which have been standardized throughout the analytical laboratories of Imperial Chemical Industries.' The three editors are members of an Analytical Chemists' Committee which the Company set up in 1928. Among the functions of this committee has been the critical examination of methods and techniques with the objects of maintaining a degree of standardization throughout the Company and of substituting new procedures for old ones wherever this seemed desirable. This is no encyclopaedic collection of works methods used throughout a large organization, but a comprehensive treatise on the art and science of chemical analysis.

The fifteen chapters of Volume I cover accuracy, manipulation, sampling, weighing, calibration and use of volumetric apparatus, measurement of temperature and pressure, measurement of density and specific gravity, determination of crystallizing point and melting point, distillation and the determination of distillation range, reagents and indicators, determination of *p*H, standardization of volumetric solutions, quantitative ultimate organic analysis, quantitative organic microanalysis, and gas analysis.

The nine chapters of Volume II cover potentiometric titration, conductometric analysis, polarography, electrodeposition, introduction to colorimetric analysis, ultra-violet and infra-red absorption spectrophotometry, emission spectrography, X-ray spectrochemical analysis, X-ray diffraction, chromatography, and application of statistical methods to chemical analysis.

The material of the first eight chapters can hardly be called new, but there can be absolutely no doubt as to the need to re-state it. It all bears the stamp of experience: one is struck also with the lucidity of the theoretical sections. The sections on quantitative ultimate organic analysis and on organic microanalysis are comprehensive and authoritative. Techniques of gas analysis, with which the first volume ends, are treated in a similarly comprehensive way.

It is in Volume II that the more recent developments appear, in which some physical property other than mass is usually measured. Most valuable of all, perhaps, is the frequent final assessment of a technique under the heading 'Uses and Limitations'. In the main it is the technique and, often in some considerable detail, the principles underlying it, to which most attention is given. It might be objected that much of this information exists elsewhere, but the inclusion is probably justifiable. In this section more of the practical examples are given of the application of the techniques, e.g. of ultra-violet absorption spectroscopy to quantitative analysis. The examples are chosen from the Company's experience.

The treatment in Volume II is a little uneven: the discussion on the uses of X-ray powder diffraction, for example, does not perhaps sufficiently indicate the outstanding value of this technique in inorganic analysis. More references to the work of Rooksby, who has been active in this field for over twenty-five years, would have emphasized this point. The chapter on emission spectrography includes only a short mention of flame excitation. In this, and in the omission of any treatment of vapour phase chromatography, the book might be said to be not quite up to date, but this clearly results from the time needed to collect, edit, and publish such a wealth of information.

The book will be of the greatest value to all analysts; if it serves also to convince young graduates that a worthwhile career is open to those of them who can accept the discipline of analysis, Imperial Chemical Industries will have done a great service.

R. C. CHIRNSIDE

CHEMISTRY OF MICRO-ORGANISMS

The Chemistry of Micro-organisms, *by A. Bracken. Pp.* 343. *Sir Isaac Pitman and Sons Limited, London.* 1955. 30*s. net.*

The compounds found in cultures of micro-organisms after growth on simple substances, such as sugars and ammonia or amino-acids, are numerous and have interested chemists and biochemists for many years. This book provides a survey of such substances and should be very useful to chemists, as it gives a full treatment of the chemistry of microbial products as well as an introduction to microbiology.

As a background to the main subject the early chapters deal briefly with the biology of bacteria, yeasts, and moulds as well as with the mechanism of enzyme action. The examples illustrating the main points are well chosen and of general interest. The book deals mainly with the extracellular products found in cultures of micro-organisms (particularly moulds), but some attention is given to the phenomenal synthetic powers of these organisms, many of which can build up complex cell components such as nucleic acids and proteins from ammonia and sugars or even carbon dioxide. The use made of micro-organisms in the commercial production of antibiotics, solvents, citric acid, vitamin $B_{12}$, and other compounds is described in some detail. Considerable attention is given to the research which led to the discovery and exploitation of penicillin, streptomycin, and other antibiotics; their chemistry also is considered in detail. A large part of the book deals with the isolation and identification of microbial pigments and of the numerous aromatic, aliphatic, and heterocyclic substances found in cultures of moulds. It seems that more is known of the chemistry of these substances than of the biochemical mechanisms which lead to their formation.

A concluding chapter provides practical details of simple microbiological techniques such as the isolation and maintenance of pure cultures; this information should be valuable for the chemist wishing to handle micro-organisms. Finally, some unsolved problems in microbiology are discussed, including the mode of action of antibiotics and the mechanism of the Gram stain.

The value of this book for reference purposes is increased by an extensive bibliography and a full index.

JUNE LASCELLES

AUTOTROPHIC BACTERIA

Biochemistry of Autotrophic Bacteria, *by H. Lees. Pp. viii* + 112. *Butterworths Scientific Publications, London.* 1955. 21*s. net.*

The biochemistry of the autotrophic bacteria is still far from understood, but is a subject which would be well served

by a critical concise monograph suitable for honours students. Unfortunately this volume presents an uneven, and sometimes superficial, discussion of present work; furthermore, it does little to relate present problems to those current in other fields of biochemistry. For example, no reference is made to the tricarboxylic acid cycle in connection with the fixation of carbon dioxide by heterotrophs, whereas the more recent, and perhaps less well established, Calvin photosynthetic cycle is given prominence. Ideas are briefly introduced but not always precisely stated. Thus the concept of a 'closed system' is introduced in one line of text and dismissed in one footnote. The author may have attempted to make the book appear comprehensible to an intelligent layman, but he has detracted much from its value to the student.

The major portion of the book is devoted to a brief description of the main groups of autotrophs; the non-photosynthetic sulphur bacteria, the hydrogen and nitrogen bacteria, and the photosynthetic bacteria. Here also the treatment is uneven; for example, Larsen's recent work with *Chlorobium* spp. is given in detail, but too little information is given about the better known purple photosynthetic bacteria.

There are some errors of fact. It suffices to awaken the unwary by calling attention to a few, such as the unusual definition of cytochromes on page 17; the statement on page 74 that thioctic acid affects photosynthesis, when in fact the quinone reaction was studied; and the omission of any reference state for the free-energy data given throughout the book.

C. P. WHITTINGHAM

### A. I. VIRTANEN

Biochemistry of Nitrogen. A Collection of Papers on the Biochemistry of Nitrogen and Related Subjects. Dedicated to A. I. Virtanen on the Occasion of his 60th Birthday. (*Editorial board; N. J. Toivonen, E. Tommila, J. Erkama, P. Roine, and J. K. Miettinen.*) *Pp.* 535. *Suomalainen Tiedeakatemia, Helsinki.* 1955. *Bound,* $12 *net; paper cover,* $10 *net.*

This collection of papers published in honour of the distinguished Finnish biochemist A. I. Virtanen contains 49 contributions on biochemical topics of current interest. The subjects include the biochemistry of amino acids, proteins, nucleic acids, porphyrins, nitrogen fixation, photosynthesis, and nitrate reduction. The field covered is rather wider than the title of the book suggests. Among the contributors are E. J. Bigwood, R. H. Burris, A. Butenandt, M. Calvin, E. B. Chain, H. von Euler, C. Fromageot, G. Hevesy, C. N. Hinshelwood, F. Lynen, L. Pauling, H. Staudinger, J. B. Sumner, H. Theorell, A. Tiselius, A. R. Todd, O. Warburg, and many other leading biochemists. While some of the articles are general reviews, like those of Todd on the synthesis of nucleotide coenzymes, Hinshelwood on mutations and adaptations in bacteria, and Staudinger on macromolecules in biology, the majority are reports on new experimental work. The book gives a fascinating cross-section of current biochemistry.

H. A. KREBS

### CHEMISTRY OF ENZYMES

Outlines of Enzyme Chemistry, *by J. B. Neilands and Paul K. Stumpf. Pp.* x + 315. *John Wiley and Sons Inc., New York; Chapman and Hall Limited, London.* 1955. 52*s. net.*

As its title suggests, this book is an outline of enzyme chemistry, but one which will be best appreciated by students already grounded in chemistry and physiology. Devoid of detail and clear in expression, it allows an overall view of enzymology to be easily and rapidly obtained.

Throughout the book the authors are consistent in their efforts to treat main concepts with brevity but not with obscurity, and at the same time to provide carefully selected references to extend their text. This method of presentation, perhaps a consequence of its origin from lecture material, makes the book easily assimilable. From an instructive viewpoint it is effective because points are amply illustrated by actual experimental data, by practical details, or by application of a principle under discussion to definite cases.

The first two parts of the book, which most students of biochemistry should relish, deal very adequately with the isolation and characterization of enzymes and the physicochemical aspects of their behaviour. The two remaining parts—'types of coenzymes and enzymes' and 'metabolic patterns'—do not flow as smoothly as the others, perhaps because of the nature of the material and the rigour of the condensation. The final chapter, by R. Stanier, deals most stimulatingly with the contributions made by microbiology to an understanding of the mechanism of enzyme synthesis.

This book will give students commencing the study of biochemistry a clear conception of the physical principles so often used in studies of enzymes, and will certainly show them how meagre is our knowledge of enzyme behaviour and how vast are the opportunities for attack upon this problem by physical and other means.

J. R. P. O'BRIEN

### CALANUS FINMARCHICUS

The Biology of a Marine Copepod, *by S. M. Marshall and A. P. Orr. Pp.* vii + 188. *Oliver and Boyd Limited, London.* 1955. 21*s. net.*

In 1770 Bishop Gunnerus of Nidaros (Trondheim) named a small crustacean taken from the sea off northern Norway. He called it *Monoculus finmarchicus*, including it in the Insecta Aptera established by Linnæus. That was the initial recognition by science of perhaps the commonest of marine animals which is a major food of pelagic fishes, including the herring, and of certain whales. The structure, physiology, life history, and habits of *Calanus finmarchicus*, as it is now called, are matters of major interest to all concerned with marine biology and fishery research. For the past thirty years *Calanus* has formed the main subject of research of the two senior members of the staff of the laboratory of the Scottish Marine Biological Association at Millport. In this book these workers have gathered together the fruits of their own labours and of all others who have studied *Calanus*: in so doing they have produced the first comprehensive account covering all aspects of the life of a marine copepod crustacean. Admirably produced, this book is a credit to both authors and publisher; it is a major addition to the literature of marine biology and of the Crustacea.

C. M. YONGE

### BIRDS OF SAURASHTRA

Birds of Saurashtra—India, *by R. S. Dharmakumarsinhji. Pp.* 561. *Published at Dil Bahar, Bhavnagar, Saurashtra, by the author.* 1955. £5 12*s.* 6*d. net.*

A consequence of the British leaving India was a decline in ornithology, since in the past nearly all research on Indian birds was carried out by Englishmen, with the recent important exception of Salim Ali. A regional survey by another Indian ornithologist is therefore most welcome. The interest of this book is primarily local. Saurashtra, the peninsula between the Gulfs of

Kutch and of Cambay, has a rich variety of shore and marsh birds; much of the inland country is dry and scrubby, but the Gir forest brings in many woodland forms. The book is primarily devoted to a description of each species, its distribution, general habits, and nesting; most of it is based on original observations, and it gives an impression of enthusiasm, competence, and reliability. There is a very short and somewhat naïve descriptive introduction. The coloured plates by Somalal Shah are sufficient for identification but are grouped with little regard for size, while related species are sometimes placed far apart. The author's valuable photographs record both behaviour and species not previously photographed. Its ample size, production, and price recall English bird-books before 1914, and suggest that it will be found chiefly in rich men's libraries. D. LACK

### PRIMATES

Primates, *by W. C. Osman Hill.* Part 1—Strepsirhini. *Pp.* xxiii + 798. Part 2—Haplorhini. *Pp. xx* + 347. *University Press, Edinburgh.* Part 1, 1953, £5 5*s. net.* Part 2, 1955, 63*s. net.*

To present, in detail, for every genus of the Primates, all that is known of the external characters, the skeleton, skull, dentition, musculature, viscera, behaviour, and distribution, is the basic aim of the survey of the Primates which Dr Osman Hill has undertaken. Already in the two volumes so far issued (on living and fossil Lemurs, Lorises, and Tarsioids) we can fully appreciate the valuable service that the author is rendering to specialists in Primatology and to many others—zoologists, palaeontologists, and anthropologists. He does more than provide a thorough catalogue of the primate genera; he has in addition taken great pains to present comprehensive definitions and specifications of all the important taxonomic categories at every level of the Primate hierarchy, Ordinal, Sub-ordinal, Familial, and Generic. These clearly written and well arranged sections of the compendium constitute in themselves a considerable contribution to the comparative anatomy and taxonomy of the Primates, even though there will be serious disagreement about some aspects of Dr Hill's classification. Thus at the outset, in deciding what constitutes a Primate, he finds no reason to go beyond the dicta of Mivart (1873), and accordingly omits the tree-shrews from the Order. There is practically no discussion of this decision, and indeed, to say that the Order 'has to be stretched almost beyond recognition' to accommodate the Tupaioidea verges on hyperbole; the exclusion of the tree-shrews robs the work of some of its usefulness as a guide to the whole evolution of the Primates. He again diverges widely from usual practice in following Pocock (1918) and promoting the Tarsioids from their 'prosimian' status into alliance with the Anthropoidea to form the Haplorhine, as distinct from the Strepsorhine, Grade. The intermediate and complicated position of the Tarsioids between Lemuroidea and Anthropoidea does of course leave room for genuine difference of taxonomic opinion, though phylogenetically it is probably true that all schools recognize the Tarsioids as following a distinctive line of development from an ancestry not far from the Basal Primate stock. Indeed, what is meant by a 'tarsian' or 'tarsioid' ancestry for the Anthropoids has never been made very clear. Dr Hill does not say much on this problem, but this is perhaps understandable, since extensive controversial discussion would mar a work of this sort. One point to be mentioned is the absence in the second volume of reference to the highly important work by Carpenter and by Hebb on anthropoid behaviour and mental development, and one hopes that this omission will not persist in subsequent volumes on the Anthropoidea or Hominoidea. Both the newly issued volumes, beautifully produced, give every promise that Dr Hill, when he has completed his task, will have provided what has been needed for some time: an up-to-date, well informed, well illustrated, and well documented source book of the Primates. J. S. WEINER

### TSETSE FLIES

The Natural History of Tsetse Flies (London School of Hygiene and Tropical Medicine Memoir No. 10), *by P. A. Buxton. Pp. xx* + 826. *H. K. Lewis and Company Limited, London.* 1955. 84*s. net.*

This is a large volume, but not because the author is prolix but because it summarizes an immense body of work mostly carried out in Africa in the last fifty years. It is written in an exceptionally lucid style, free from all but the essential technical terms. It is very well produced, amply documented, with an extensive index and excellent illustrations. It is almost completely free of misprints. The work is encyclopaedic in conception, and besides discussing every aspect of tsetse biology deals, at least briefly, with such subjects as the climate and vegetation of Africa and the species problem in Trypanosomes. There are many topics on which all entomologists will find it a valuable work of reference. Apart from the importance of tsetse to the health and welfare of man, the book is also an admirable illustration of how a large and complex biological problem can be tackled in the field.

The author does not in general refer to literature published after the end of 1952. At about that time papers appeared of great importance to two chapters in the book. It is probable that the attempts to relate tsetse populations to climate should be reconsidered. The numbers of flies form a series with an internal correlation, and this greatly complicates any attempt to analyse their relation to climate. The complex correlations between different climatic factors, not all of which have been measured, is a further difficulty. The study of populations by marking and recapture, a method largely developed by students of the tsetse, turns out to be more complicated than was supposed. Bailey (1951–52) has shown that the usual methods involve a bias which at best invalidates tests of significance. There appear to be two topics which could be studied in greater detail in the tsetse fly. One is the cytology, which might well confirm or correct the present taxonomy. The other is the study of fly behaviour by modern methods; it is possible that this would make it easier to understand the process of habitat-selection. O. W. RICHARDS

# Some books received

(*Note.* Mention of a book on this page does not preclude subsequent review.)

### BIOLOGY

The Cytology and Life-History of Bacteria (*second edition*), *by K. A. Bisset. Pp. xii + 164. E. and S. Livingstone Limited, Edinburgh and London.* 1955. *25s. net.*

School Nature Study, Volume 50, Nos. 198–201. The Journal of the School Nature Study Union. *Headley Brothers Limited, London. Annual Subscription 10s. net.*

Some Aspects of Life in Fresh Water, *by E. J. Popham. Pp viii + 123. William Heinemann Limited, London.* 1955. *6s. net.*

Traité de Paléontologie, Volume 5. Amphibiens, Reptiles, Oiseaux, *edited by J. Piveteau. Pp. 1113. Masson et Cie, Paris.* 1955. *Paper covers, Fcs. 12 000; bound, Fcs. 12 800 net.*

### BOTANY

The Annual Report of the Agricultural and Horticultural Research Station (The National Fruit and Cider Institute), Long Ashton, Bristol, 1954. *Pp. 207. University of Bristol,* 1955. *15s. net.*

### CHEMISTRY

Biochemical Preparations, Volume 4, *edited by W. W. Westerfeld. Pp. vii + 108. John Wiley and Sons Inc., New York; Chapman and Hall Limited, London.* 1955. *30s. net.*

Catalysis, Volume 3, *edited by P. H. Emmett. Pp. vii + 504. Reinhold Publishing Corporation, New York; Chapman and Hall Limited, London.* 1955. *100s. net.*

Cole's Practical Physiological Chemistry, *by E. Baldwin and D. J. Bell. Pp. x + 263. W. Heffer and Sons, Cambridge.* 1955. *21s. net.*

Cosmologie et Chimie, *by A. Dauvillier. Pp. viii + 215. Presses Universitaires de France, Paris.* 1955. *Fcs. 900 net.*

Hydrogen Ions, Volume 1 (*fourth edition*), *by H. T. S. Britton. Pp. xix + 476. Chapman and Hall Limited, London.* 1955. *70s. net.*

Rechentafeln zur Chemischen Elementar-Analyse, *by R. Krzikalla. Pp. 232. Verlag Chemie G.m.b.H., Weinheim/Bergstr.* 1956. *DM 12 net.*

Spectrochemical Abstracts, Volume 4, 1946–1951, *by E. H. S. van Someren and F. Lachman. Pp. 179. Hilger and Watts Limited, London.* 1955. *30s. net.*

Synthetic Ion-Exchangers, *by G. H. Osborn. Pp. ix + 419. Chapman and Hall Limited, London.* 1955. *30s. net.*

### GENERAL

Erlanger Forschungen, Volume 1, Series B: Naturwissenschaften (Johann Gottlob Lehman (1719–1767)), *by Bruno v. Freberg. Pp. 159. Universitätsbibliothek, Erlangen.* 1955. *DM 12.80 net.*

Fifth Symposium (International) on Combustion. *Pp. xxvi + 802. Reinhold Publishing Corporation, New York; Chapman and Hall Limited, London.* 1955. *120s. net.*

Ludwig Boltzmann, *by E. Broda. Pp. viii + 152. Franz Deuticke, Vienna.* 1955. *Paper covers, DM 9.50 net; linen, DM 11 net.*

Plastics for Corrosion-Resistant Applications, *by R. B. Seymour and R. H. Steiner. Pp. xvi + 423. Reinhold Publishing Corporation, New York; Chapman and Hall Limited, London.* 1955. *60s. net.*

Praktische Edelstein-Prüfung, *by B. W. Anderson. Pp. 262. Ruhle-Diebener-Verlag K.G., Stuttgart.* 1955. *DM 24.60 net.*

Research Films in Biology, Anthropology, Psychology and Medicine, *by A. R. Michaelis. Pp. xvi + 490. Academic Press Inc., New York.* 1955. *$10 net.*

Titanium—Metallurgy of the Rarer Metals, No. 4, *by A. D. McQuillan and M. K. McQuillan. Pp. xix + 466. Butterworths Scientific Publications, London.* 1956. *56s. net.*

### GEOLOGY

Stratigraphic Geology, *by M. Gignoux, translated by G. G. Woodford. Pp. xvi + 682. W. H. Freeman and Company, San Francisco; Bailey Brothers and Swinfen Limited, London.* 1955. *80s. net.*

### MATHEMATICS

Experimental Design and its Statistical Basis, *by D. J. Finney. Pp. xi + 169. Cambridge University Press, London.* 1955. *30s. net.*

### MEDICINE

Basi Teoriche e Sperimentali per una Patogenesi enzimatica dell'Arteriosclerosi, *by A. Cali, Pp. 107. Libreria Natale Simonelli Editrice, Perugia.* 1955. *Lire 800 net.*

### PHYSICS

Errors of Observation and their Treatment, *by J. Topping. Pp. 119. The Institute of Physics, London.* 1955. *5s. net.*

Humidity, *by H. L. Penman. Pp. 71. The Institute of Physics, London.* 1955. *5s. net.*

An Introduction to Reactor Physics, *by D. J. Littler and J. F. Raffle. Pp. viii + 196. Pergamon Press Limited, London.* 1955. *25s. net.*

Introductory Nuclear Physics (*second edition*), *by D. Halliday. Pp. ix + 493. John Wiley and Sons Inc., New York; Chapman and Hall Limited, London.* 1955. *60s. net.*

Les Dislocations et la Croissance des Cristaux, *by W. Dekeyser and S. Amelinckx. Pp. viii + 184. Masson et Cie, Paris.* 1955. *Fcs 2000 net.*

Low Temperature Physics, *by L. C. Jackson. Pp. viii + 158. Methuen and Company Limited, London; John Wiley and Sons Inc., New York.* 1955. *10s. 6d. net.*

Microstructures of Diamond Surfaces, *by S. Tolansky. Pp. viii + 67. N.A.G. Press Limited, London.* 1955. *40s. net.*

Niels Bohr and the Development of Physics, *edited by W. Pauli, L. Rosenfeld, and V. Weisskopf. Pp. vii + 195. Pergamon Press Limited, London.* 1955. *30s. net.*

Nuclear Magnetic Resonance, *by E. R. Andrew. Pp. xi + 265. Cambridge University Press, London.* 1955. *35s. net.*

Nuclear Physics, Volume 1, No. 1, *edited by L. Rosenfeld. Pp. 72. North-Holland Publishing Company, Amsterdam.* 1956. *Subscription price for volume (700 pages) 106s. net.*

Nuclear and Radiochemistry, *by G. Friedlander and J. W. Kennedy. Pp. ix + 468. John Wiley and Sons Inc., New York; Chapman and Hall Limited, London.* 1955. *60s. net.*

Physics of Fibres, *by H. J. Woods. Pp. 100. The Institute of Physics, London.* 1955. *30s. net.*

### ZOOLOGY

Soil Zoology (Proceedings of the University of Nottingham Second Easter School in Agricultural Science, 1955), *edited by D. K. McE. Kevan. Pp. xiv + 512. Butterworths Scientific Publications, London; Academic Press Inc., New York.* 1955. *55s. net.*

The Wildfowl Trust, Seventh Annual Report, 1953–1954, *edited by P. Scott and H. Boyd. Pp. 235. The Country Life Limited, London.* 1955. *10s. net.*

# Notes on contributors

ALFRED LANDÉ,
*Ph.D.*,

Was born at Elberfeld, Germany, in 1888, and studied at Marburg, Göttingen, and Munich: he was a pupil of Max Born and Arnold Sommerfeld. From 1920 to 1922 he was Privatdozent at Frankfurt and from 1922 to 1931 professor of physics at Tübingen. Since 1931 he has been professor of physics at Ohio State University. His researches have been on the vector model of atomic structure, spectral lines, the Zeeman effect, and multiplet theory. He is the author of several books on the principles of quantum theory; his present article is based on the Fulbright Lectures he gave in Italy in 1954.

J. W. S. PRINGLE,
*Sc.D., M.B.E., F.R.S.*,

Was born in Manchester in 1912 and was educated at Winchester and at King's College, Cambridge. In 1938 he became University Demonstrator in Zoology in the University of Cambridge, but in the following year left for war service with the Telecommunications Research Establishment, and subsequently with the Ministry of War Transport. Since 1945 he has been University Lecturer in Zoology at Cambridge and Fellow of Peterhouse. His research has been on the physiology of the nerves and muscles of insects. In 1953 he visited the University of Ceylon as Leverhulme Research Fellow and carried out work on insect song.

A. T. JAMES,
*B.Sc., Ph.D.*,

Was born in Cardiff in 1922 and was educated at University College School, Hampstead. While working with Kodak Limited he continued his studies at the Northern Polytechnic, London, from which he won a scholarship to University College, London. After some research on antimalarial drugs at Bedford College, London, he carried out at the Lister Institute, with R. L. M. Synge, an investigation of the structure of gramicidin. In 1950 he joined A. J. P. Martin at the National Institute for Medical Research and with him developed the technique of gas-liquid chromatography, which he is now using for the study of fat metabolism.

LORD ROTHSCHILD,
*G.M., Sc.D., F.R.S.*,

Was born in London in 1910, and was educated at Harrow and at Trinity College, Cambridge, of which he became a Fellow. During the war of 1939–45 he was a bomb disposal specialist in the British Intelligence Service. Is now Assistant Director of Research in the Department of Zoology, Cambridge, doing research on the physiology of eggs, spermatozoa, and fertilization. He has been chairman of the Agricultural Research Council since 1948.

L. ESSEN,
*D.Sc., Ph.D., A.M.I.E.E.*,

Was born in 1908 and was educated at High Pavement School and University College, Nottingham. He joined the National Physical Laboratory in 1929 and has been responsible for the standards of frequency there since 1931. The quartz ring oscillator which he developed is now used at many observatories and standardizing laboratories throughout the world. During the war he developed measurement techniques for microwave frequencies and has since applied these techniques to the determination of fundamental constants and in particular to the construction of an atomic standard of frequency and time interval.

SIR ROBERT ROBINSON,
*O.M., M.A., D.Sc., LL.D., F.R.S.*,

Was born in 1886 and was educated at Fulneck School, near Leeds, and the University of Manchester. Appointed professor of organic chemistry in the University of Sydney in 1912, he has since held several professorial chairs, and from 1930 to 1955 was Waynflete Professor of Chemistry in the University of Oxford. He is an Honorary Fellow of Magdalen College. He is now associated with the Shell Chemical Company Limited. His scientific work has been concerned with the structure and synthesis of such natural products as alkaloids and anthocyanins, and with the application of electronic theories to the mechanism of reaction. He has been president of The Royal Society and of The Chemical Society, and is a medallist of these and many other learned bodies. He was president of the British Association for the Advancement of Science in 1955. He is chairman of the Perkin Centenary Committee.

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